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AGRICULTURAL RESEARCH INSTITUTE
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PHILOSOPHICAL
TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCV.

PART I.

LONDON,

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ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

ERRATA.

Page 4. line 5 from the bottom, *for* accipenser, *read* acipenser.
11. — 20, *for* their, *read* its.
26. — 7, *read* 6 *ounces* of water.
76. — last but one, *for* ,0054, *read* ,054.

The President and Council of the Royal Society adjudged, for the Year 1804, the Medal on Sir Godfrey Copley's Donation, to Smithson Tennant, Esq. F. R. S. for his various Chemical Discoveries, communicated to the Society, and printed in several Volumes of the Transactions.

And they adjudged the Gold and Silver Medals, on the Donation of Benjamin Count of Rumford, to Mr. John Leslie of Largo, for his Experiments on Heat, published in his Work, entitled an Experimental Inquiry into the Nature and Propagation of Heat.

PHILOSOPHICAL

TRANSACTIONS.

I. *The Croonian Lecture on muscular Motion.* *By* Anthony Carlisle, *Esq. F. R. S.*

Read November 8, 1804.

ANIMAL physiology has derived several illustrations and additions, from the institution of this Lecture on muscular Motion; and the details of anatomical knowledge have been considerably augmented by descriptions of muscular parts before unknown.

Still, however, many of the phenomena of muscles remain unexplained, nor is it to be expected that any sudden insulated discovery shall solve such a variety of complicated appearances.

Muscular motion is the first sensible operation of animal life: the various combinations of it sustain and carry on the multiplied functions of the largest animals: the temporary cessation of this motive faculty is the suspension of the living powers, its total quiescence is death.

By the continuance of patient, well directed researches, it is reasonable to expect much important evidence on this subject;

and, from the improved state of collateral branches of knowledge, together with the addition of new sources, and methods of investigation, it may not be unreasonable to hope for an ultimate solution of these phenomena, no less complete, and consistent, than that of any other desideratum in physical science.

The present attempt to forward such designs is limited to circumstances which are connected with muscular motion, considered as causes, or rather as a series of events, all of which contribute, more or less, as conveniences, or essential requisites, to the phenomena; the details of muscular applications being distinct from the objects of this lecture.

No satisfactory explanation has yet been given of the state or changes which obtain in muscles during their contractions or relaxations, neither are their corresponding connections with the vascular, respiratory, and nervous systems, sufficiently traced. These subjects are therefore open for the present enquiry, and although I may totally fail in this attempt to elucidate any one of the subjects proposed, nevertheless I shall not esteem my labour useless, or the time of the Royal Society altogether unprofitably consumed, if I succeed in pointing out the way to the future attainment of knowledge so deeply interesting to mankind.

The muscular parts of animals are most frequently composed of many substances, in addition to those which are purely muscular. In this gross state, they constitute a flexible, compressible solid, whose texture is generally fibrous, the fibres being compacted into fasciculi, or bundles of various thickness. These fibres are elastic during the contracted state of muscles after death, being capable of extension to more than one-fifth of their length, and of returning again to their former state of contraction.

This elasticity, however, appears to belong to the enveloping reticular or cellular membrane, and it may be safely assumed that the intrinsic matter of muscle is not elastic.

The attraction of cohesion, in the parts of muscle, is strongest in the direction of the fibres, it being double that of the contrary, or transverse direction.

When muscles are capable of reiterated contractions and relaxations, they are said to be alive, or to possess irritability. This quality fits the organ for its functions. Irritability will be considered, throughout the present lecture, as a quality only.

When muscles have ceased to be irritable, their cohesive attraction in the direction of their fibres is diminished, but it remains unaltered in the transverse direction.

The hinder limbs of a frog attached to the pelvis being stripped of the skin, one of them was immersed in water at 115° of FAHRENHEIT, during two minutes, when it ceased to be irritable. The thigh bones were broken in the middle, without injuring the muscles, and a scale affixed to the ancle of each limb: a tape passed between the thighs was employed to suspend the apparatus. Weights were gradually introduced into each scale, until, with five pounds avoirdupois, the dead thigh was ruptured across the fleshy bellies of its muscles.

The irritable thigh sustained six pounds weight avoirdupois, and was ruptured in the same manner. This experiment was repeated on other frogs, where one limb had been killed by a watery solution of opium, and on another where essential oil of cherry laurel* was employed: in each experiment, the irritable limb sustained a weight one-sixth heavier than the dead limb.

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* Distilled oil from the leaves of the *Prunus Lauro-cerasus*.

It may be remarked, in confirmation of these experiments, that when muscles act more powerfully, or more rapidly, than is equal to the strength of the sustaining parts, they do not usually rupture their fleshy fibres, but break their tendons, or even an intervening bone, as in the instances of ruptured tendo Achillis, and fractured patella. Instances have however occurred, wherein the fleshy bellies of muscles have been lacerated by spasmodic actions; as in tetanus the recti abdominis have been torn asunder, and the gastrocnemii in cramps; but in those examples it seems that either the antagonists produce the effect, or the over-excited parts tear the less excited in the same muscle. From whence it may be inferred, that the attraction of cohesion in the matter of muscle is considerably greater during the act of contracting, than during the passive state of tone, or irritable quiescence, a fact which has been always assumed by anatomists from the determinate forces which muscles exert.

The muscular parts of different classes of animals vary in colour and texture, and not unfrequently those variations occur in the same individual.

The muscles of fishes and vermes are often colourless, those of the mammalia and birds being always red: the amphibia, the accipenser, and squalus genera, have frequently both red and colourless muscles in the same animal.

Some birds, as the black game,* have the external pectoral muscles of a deep red colour, whilst the internal are pale.

In texture, the fasciculi vary in thickness, and the reticular membrane is in some parts coarse, and in others delicate: the heart is always compacted together by a delicate reticular membrane, and the external glutæi by a coarser species.

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* *Tetrao tetrix.* LIN.

An example of the origin of muscle is presented in the history of the incubated egg, but whether the rudiments of the punctum saliens be part of the cicatricula organised by the parent, or a structure resulting from the first process of incubation, may be doubtful: the little evidence to be obtained on this point seems in favour of the former opinion; a regular confirmation of which would improve the knowledge of animal generation by shewing that it is gemmiferous. There are sufficient analogies of this kind in nature, if reasoning from analogies were proper for the present occasion.

The punctum saliens, during its first actions, is not encompassed by any fibres discoverable with microscopes, and the vascular system is not then evolved, the blood flowing forwards, and backwards, in the same vessels. The commencement of life in animals of complex structure is, from the preceding fact, like the ultimate organization of the simpler classes.

It is obvious that the muscles of birds are formed out of the albumen ovi, the vitellus, and the atmospheric air, acted upon by a certain temperature. The albumen of a bird's egg is wholly consumed during incubation, and the vitellus little diminished, proving that the albumen contains the principal elementary materials of the animal thus generated; and it follows that the muscular parts, which constitute the greater proportion of such animals when hatched, are made out of the albumen, a small portion of the vitellus, and certain elements, or small quantities of the whole compound of the atmosphere.

The muscles of birds are not different, in any respect, from those of quadrupeds of the class of mammalia.

The anatomical structure of muscular fibres is generally complex, as those fibres are connected with membrane, blood-vessels, nerves, and lymphæducts; which seem to be only appendages of convenience to the essential matter of muscle.

A muscular fibre, duly prepared by washing away the adhering extraneous substances, and exposed to view in a powerful microscope, is undoubtedly a solid cylinder, the covering of which is reticular membrane, and the contained part a pulpy substance irregularly granulated, and of little cohesive power when dead.

A difficulty has often subsisted among anatomists concerning the ultimate fibres of muscles; and, because of their tenuity, some persons have considered them infinitely divisible, a position which may be contradicted at any time, by an hour's labour at the microscope.

The arteries arboresce copiously upon the reticular coat of the muscular fibre, and in warm-blooded animals these vessels are of sufficient capacity to admit the red particles of blood, but the intrinsic matter of muscle, contained within the ultimate cylinder, has no red particles.

The arteries of muscles anastomose with corresponding veins; but this course of a continuous canal cannot be supposed to act in a direct manner upon the matter of muscle.

The capillary arteries terminating in the muscular fibre must alone effect all the changes of increase in the bulk, or number, of fibres, in the replenishment of exhausted materials, and in the repair of injuries; some of these necessities may be supposed to be continually operating. It is well known, that the circulation of the blood is not essential to muscular action; so that the mode of distribution of the blood vessels, and the differences in their size, or number, as applied to muscles, can only be adaptations to some special convenience.

Another prevalent opinion among anatomists, is the infinite extension of vascularity, which is contradicted in a direct manner by comparative researches. The several parts of a quadruped are sensibly more or less vascular, and of different contextures; and, admitting that the varied diameter of the blood vessels disposed in each species of substance, were to be constituted by the gross sensible differences of their larger vessels only, yet, if the ultimate vessels were in all cases equally numerous, then the sole remaining cause of dissimilarity would be in the compacting of the vessels. The vasa vasorum of the larger trunks furnish no reason, excepting that of a loose analogy, for the supposition of vasa vasorum extended without limits. Moreover, the circulating fluids of all animals are composed of water, which gives them fluidity, and of animalised particles of defined configuration and bulk; it follows that the vessels through which such fluids are to pass, must be of sufficient capacity for the size of the particles, and that smaller vessels could only filtrate water devoid of such animal particles: a position repugnant to all the known facts of the circulation of blood, and the animal economy.

The capillary arteries which terminate in the muscular fibre, must be secretory vessels for depositing the muscular matter, the lymphæducts serving to remove the superfluous extravasated watery fluids, and the decayed substances which are unfit for use.

The lymphæducts are not so numerous as the blood vessels, and certainly do not extend to every muscular fibre: they appear to receive their contained fluids from the intersticial spaces formed by the reticular or cellular membrane, and not from the projecting open ends of tubes, as is generally represented. This mode of receiving fluids out of a cellular structure, and conveying them into cylindrical vessels, is exemplified in the corpora cavernosa, and corpus spongiosum penis, where arterial blood is poured into cellular or reticular cavities, and from thence it passes into common veins by the gradual coarctation of the cellular canals.

In the common green turtle, the lacteal vessels universally arise from the loose cellular membrane, situated between the internal spongy coat of the intestines and the muscular coat. The cellular structure may be filled from the lacteals, or the lacteals from the cellular cavities. When injecting the smaller branches of the lymphæducts retrograde in an œdematous human leg, I saw, very distinctly, three orifices of these vessels terminating in the angles of the cells, into which the quicksilver trickled. The preparation is preserved, and a drawing of the appearance made at the time. It was also proved, by many experiments, that neither the lymphæducts, nor the veins, have any valves in their minute branches.

The nerves of voluntary muscles separate from the same bundles of fibrils with the nerves which are distributed in the skin, and other parts, for sensation; but a greater proportion of nerve is appropriated to the voluntary muscles, than to any other substances, the organs of the senses excepted.

The nerves of volition all arise from the parts formed by the junction of the two great masses of the brain, called the Cerebrum and Cerebellum, and from the extension of that substance throughout the canal of the vertebræ. Another class of muscles, which are not subject to the will, are supplied by peculiar nerves; they are much smaller, in proportion to the bulk of the parts on which they are distributed, than those of the voluntary muscles; they contain less of the white opaque medullary substance than the other nerves, and unite their fibrils, forming numerous anastomoses with all the other nerves of the body, excepting those appropriated to the organs of the senses. There are enlargements at several of these junctions, called Ganglions, and which are composed of a less proportion of the medullary substance, and their texture is firmer than that of ordinary nerves.

The terminal extremities of nerves have been usually considered of unlimited extension; by accurate dissection however, and the aid of magnifying glasses, the extreme fibrils of nerves are easily traced as far as their sensible properties, and their continuity extends. The fibrils cease to be subdivided whilst perfectly visible to the naked eye, in the voluntary muscles of large animals, and the spaces they occupy upon superficies where they seem to end, leave a remarkable excess of parts unoccupied by those fibrils. The extreme fibrils of nerves lose their opacity, the medullary substance appears soft and transparent, the enveloping membrane becomes pellucid, and the whole fibril is destitute of the tenacity necessary to preserve its own distinctness; it seems to be diffused and mingled with the substances in which it ends. Thus the ultimate terminations of nerves for volition, and ordinary sensation, appear to be in the reticular membrane, the common covering of all the different substances in an animal body, and the connecting medium of all dissimilar parts.

By this simple disposition, the medullary substance of nerve is spread through all organized, sensible, or motive parts, forming a continuity which is probably the occasion of sympathy. Peculiar nerves, such as the first and second pairs, and the portio mollis of the seventh, terminate in an expanse of medullary substance which combines with other parts and membranes, still keeping the sensible excess of the peculiar medullary matter.

The peculiar substance of nerves must in time become inefficient; and, as it is liable to injuries, the powers of restoration, and repair, are extended to that material. The reunion of nerves after their division, and the reproduction after part of a nerve has been cut away, have been established by decisive experiments. Whether there is any new medullary substance employed to fill up the break, and, if so, whether the new substance be generated at the part, or protruded along the nervous theca from the brain, are points undetermined: the history of the formation of a fœtus, the structure of certain monsters, and the organization of simple animals, all seem to favour the probability, that the medullary matter of nerves is formed at the parts where it is required, and not in the principal seat of the cerebral medulla.

This doctrine, clearly established, would lead to the belief of a very extended commixture of this peculiar matter in all the sensible and irritable parts of animals, leaving the nerves in their limited distribution, the simple office of conveying impressions from the two sentient masses with which their extremities are connected. The most simple animals in whom no visible appearances of brain or nerves are to be found, and no fibrous arrangement of muscles, may be considered of this description: Mr. John Hunter appeared to have had some incomplete notions upon this subject, which may be gathered from his representation of a materia vitæ in his Treatise on the Blood, &c. Perhaps it would be more proper to distinguish the peculiar matter of muscle by some specific term, such, for example, as materia contractilis.

A particular adaptation for the nerves which supply the electrical batteries of the torpedo, and gymnotus, is observable, on the exit of each from the skull; over which there is a firm cartilage acting as a yoke, with a muscle affixed to it, for the obvious purpose of compression: so that a voluntary muscle probably governs the operations of the battery.

The matter of the nerves, and brain, is very similar in all the different classes of animals.

The external configuration of animals is not more varied than their internal structure.

The bulk of an animal, the limitation of its existence, the medium in which it lives, and the habits it is destined to pursue, are each, and all of them, so many indications of the complexity or simplicity of their internal structure. It is notorious that the number of organs, and of members, is varied in all the different classes of animals; the vascular and nervous systems, the respiratory, and digestive organs, the parts for procreation, and the instruments of motion, are severally varied, and adapted to the condition of the species. This modification of anatomical structure is extended in the lowest tribes of animals, until the body appears to be one homogeneous substance. The cavity for receiving the food is indifferently the internal, or external surface, for they may be inverted, and still continue to digest food; the limbs or tentacula may be cut off, and they will be regenerated without apparent inconvenience to the individual: the whole animal is equally sensible, equally irritable, equally alive: its procreation is gemmiferous. Every part is pervaded by the nutritious juices, every part is acted upon by the respiratory influence, every part is equally capable of motion, and of altering its figure in all directions, whilst neither blood-vessels, nerves, nor muscular fibres, are discoverable by any of the modes of investigation hitherto instituted.

From this abstract animal (if such a term may be admitted) up to the human frame, the variety of accessory parts, and of organs by which a complicated machinery is operated, exhibit infinite marks of design, and of accommodations to the purposes which fix the order of nature.

In the more complicated animals, there are parts adapted for trivial conveniences, much of their materials not being alive, and the entire offices of some liable to be dispensed with. The water transfused throughout the intersticial spaces of the animal fabric, the combinations with lime in bones, shells, and teeth; the horns, hoofs, spines, hairs, feathers, and cuticular coverings, are all of them, or the principal parts of their substance, extra-vascular, insensible, and unalterable by the animal functions after they are completed. I have formed an opinion, grounded on extensive observation, that many more parts of animal bodies may be considered as inanimate substances; even the reticular membrane itself seems to be of this class, and tendons, which may be the condensed state of it; but these particulars are foreign to the present occasion.

The deduction now to be made, and applied to the history of muscular motion, is, that animated matter may be connected with inanimate; this is exemplified in the adhesions of the muscles of multi-valve, and bi-valve shell fish, to the inorganic shell, the cancer Bernhardus to the dead shells of other animals, and in the transplantation of teeth. All of which, although somewhat contrary to received opinion, have certainly no degree of vascularity, or vital connection with the inhabitant; these shells being liable to transudations of cupreous salts and other poisonous substances, whilst the animal remains uninjured. A variety of proofs to the same effect might be adduced, but it would be disrespectful to this learned Body to urge any farther illustrations on a subject so obvious.

The effects of subdivision, or comminution of parts among the complicated organized bodies, is unlike that of mineral bodies: in the latter instance, the entire properties of the substance are retained, however extensive the subdivision; in the former substances, the comminution of parts destroys the essential texture and composition, by separating the gross arrangements of structure upon which their specific properties depend. From similar causes it seems to arise, that animals of minute bulk are necessarily of simple structure: size alone is not, however, the sole cause of their simple organization, because examples are sufficiently numerous wherein the animal attains considerable bulk, and is of simple structure, and *vice versâ:* but, in the former, the medium in which they live, and the habits they assume, are such as do not require extensive appendages, whilst the smaller complex animals are destined to more difficult, and more active exertions. It may be assumed however, as an invariable position, that the minutest animals are all of simple organization.

Upon a small scale, life may be carried on with simple materials; but the management, and provisions for bulky animals, with numerous limbs, and variety of organs, and appendages of convenience, are not effected by simple apparatus; thus, the skeleton which gives a determinate figure to the species, supports its soft parts, and admits of a geometrical motion, is placed interiorly, where the bulk of the animal admits of the bones being sufficiently strong, and yet light enough for the moving powers; but the skeleton is placed externally, where the body is reduced below a certain magnitude, or where the movements of the animal are not to be of the floating kind: in which last case the bulk is not an absolute cause. The examples of testaceous vermes, and coleopterous, as well as most other insects, are universally known.

The opinion of the muscularity of the crystalline lens of the eye, so ingeniously urged by a learned member of this Society, is probably well founded; as the arrangement of radiating lines of the matter of muscle, from the centre to the circumference of the lens, and these compacted into angular masses, would produce specific alterations in its figure.

This rapid sketch of the history of muscular structure has been obtruded before the Royal Society to introduce the principal experiments, and reasonings which are to follow: they are not ordered with so much exactness as becomes a more deliberate essay, but the intention already stated, and the limits of a lecture are offered as the apology.

Temperature has an essential influence over the actions of muscles, but it is not necessary that the same temperature should subsist in all muscles during their actions; neither is it essential that all the muscular parts of the same animal should be of uniform temperatures for the due performance of the motive functions.

It appears that all the classes of animals are endowed with some power of producing thermometrical heat, since it has been so established in the amphibia, pisces, vermes, and insecta, by Mr. John Hunter; a fact which has been verified to my own experience; the term "cold-blooded" is therefore only relative. The ratio of this power is not, however, in these examples, sufficient to preserve their equable temperature in cold climates, so that they yield to the changes of the atmosphere, or the medium in which they reside, and most of them become torpid, approaching to the degree of freezing water. Even the mammalia, and aves, possess only a power of resisting certain limited degrees of cold; and their surfaces, as well as their limbs, being distant from the heart, and principal blood-vessels, the muscular parts so situated are subject to considerable variations in their temperature, the influence of which is known.

In those classes of animals which have little power of generating heat, there are remarkable differences in the structure of their lungs, and in the composition of their blood, from the mammalia and aves.

Respiration is one of the known causes which influences the temperatures of animals: where these organs are extensive, the respirations are performed at regular intervals, and are not governed by the will, the whole mass of blood being exposed to the atmosphere in each circulation. In all such animals living without the tropics, their temperature ranges above the ordinary heat of the atmosphere, their blood contains more of the red particles than in the other classes, and their muscular irritability ceases more rapidly after violent death.

The respirations of the animals denominated "cold-blooded," are effected differently from those of high temperature; in some of them, as the amphibia of LINNÆUS, the lungs receive atmospheric air, which is arbitrarily retained in large cells, and not alternately, and frequently changed. The fishes, and the testaceous vermes, have lungs which expose their blood to water, but whether the water alone, or the atmospheric air mingled with it, furnish the changes in the pulmonary blood, is not known.

In most of the genera of insects, the lungs are arborescent tubes containing air, which, by these channels, is carried to every vascular part of the body. Some of the vermes of the simpler construction have no appearance of distinct organs, but the respiratory influence is nevertheless essential to their existence, and it seems to be effected on the surface of the whole body.

In all the colder animals, the blood contains a smaller proportion of the red colouring particles than in the mammalia, and aves; the red blood is limited to certain portions of the body, and many animals have none of the red particles.

The following animals were put into separate glass vessels, each filled with a pound weight of distilled water, previously boiled to expel the air, and the vessels inverted into quicksilver; *viz.* one gold fish, one frog, two leeches, and one fresh-water muscle.* These animals were confined for several days, and exposed to the sun in the day time, during the month of January, the temperature being from 43° to 48°, but no air bubbles were produced in the vessels, nor any sensible diminution of the water. The frog died on the third day, the fish on the fifth, the leeches on the eighth, and the fresh-water muscle on the thirteenth. This unsuccessful experiment was made with the hope of ascertaining the changes produced in water by the respiration of aquatic animals, but the water had not undergone any chemical alteration.

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* *Mytilus Anatinus.*

Animals of the class mammalia which hybernate, and become torpid in the winter, have at all times a power of subsisting under a confined respiration, which would destroy other animals not having this peculiar habit. In all the hybernating mammalia there is a peculiar structure of the heart, and its principal veins; the superior cava divides into two trunks; the left, passing over the left auricle of the heart, opens into the inferior part of the right auricle, near to the entrance of the vena cava inferior. The veins usually called azygos, accumulate into two trunks, which open each into the branch of the vena cava superior, on its own side of the thorax. The intercostal arteries and veins in these animals are unusually large.

This tribe of quadrupeds have the habit of rolling up their bodies into the form of a ball during ordinary sleep, and they invariably assume the same attitude when in the torpid state: the limbs are all folded into the hollow made by the bending of the body; the clavicles, or first ribs, and the sternum, are pressed against the fore part of the neck, so as to interrupt the flow of blood which supplies the head, and to compress the trachea: the abdominal viscera, and the hinder limbs are pushed against the diaphragm, so as to interrupt its motions, and to impede the flow of blood through the large vessels which penetrate it, and the longitudinal extension of the cavity of the thorax is entirely obstructed. Thus a confined circulation of the blood is carried on through the heart, probably adapted to the last weak actions of life, and to its gradual recommencement.

This diminished respiration is the first step into the state of torpidity; a deep sleep accompanies it; respiration then ceases altogether; the animal temperature is totally destroyed, coldness and insensibility take place, and finally the heart concludes its motions, and the muscles cease to be irritable. It is worthy of remark that a confined air, and a confined respiration, ever precede these phenomena: the animal retires from the open atmosphere, his mouth and nostrils are brought into contact with his chest, and enveloped in fur; the limbs become rigid, but the blood never coagulates during the dormant state. On being roused, the animal yawns, the respirations are fluttering, the heart acts slowly and irregularly, he begins to stretch out his limbs, and proceeds in quest of food. During this dormancy, the animal may be frozen, without the destruction of the muscular irritability, and this always happens to the garden snail,* and to the chrysalides of many insects during the winter of this climate.

The loss of motion and sensation from the influence of low temperature, accompany each other, and the capillaries of the vascular system appear to become contracted by the loss of animal heat, as in the examples of numbness from cold. Whether the cessation of muscular action be owing to the impeded influence of the nerves, or to the lowered temperature of the muscles themselves, is doubtful; but the known influence of cold upon the sensorial system, rather favours the supposition that a certain temperature is necessary for the transmission of nervous influence, as well as sensation.

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* *Helix nemoralis.*

The hybernating animals require a longer time in drowning than others. A full grown hedge-hog was submersed in water at 48°, and firmly retained there; air-bubbles began instantly to ascend, and continued during four minutes; the animal was not yet anxious for its liberty. After seven minutes it began to look about, attempting to escape; at ten minutes it rolled itself up, only protruding the snout, which was hastily retracted on being touched with the finger, and even the approach of the finger caused it to retract. After fifteen minutes complete submersion, the animal still remained rolled up, and withdrew its nose on being touched. After remaining thirty minutes under water, the animal was laid upon flannel, in an atmosphere of 62°, with its head inclined downwards; it soon began to relax the sphincter muscle which contracts the skin, slow respirations commenced, and it recovered entirely, without artificial aid, after two hours. Another hedge-hog submersed in water at 94°, remained quiet until after five minutes; about the eighth minute it stretched itself out, and expired at the tenth. It remained relaxed, and extended, after the cessation of the vital functions; and its muscles were relaxed, contrary to those of the animal drowned in the colder water.

The irritability of the heart is inseparably connected with respiration. Whenever the inhaled gas differs in its properties from the common atmosphere, the muscular and sensible parts of the system exhibit the change: the actions of the heart are altered or suspended, and the whole muscular and sensorial systems partake of the disorder: the temperature of animals, as before intimated, seems altogether dependant on the respiratory functions, although it still remains uncertain in what manner this is effected.

The blood appears to be the medium of conveying heat to the different parts of the body; and the changes of animal temperature in the same individual at various times, or in its several parts, are always connected with the degree of rapidity of the circulation. It is no very wide stretch of physiological deduction to infer, that this increased temperature is produced by the more frequent exposure of the mass of blood to the respiratory influence, and the short time allowed in each circuit for the loss of the acquired heat.

The blood of an animal is usually coagulated immediately after death, and the muscles are contracted; but, in some peculiar modes of death, neither the one, nor the other of these effects are produced: with such exceptions, the two phenomena are concomitant.

A preternatural increase of animal heat delays the coagulation of the blood, and the last contractions of the muscles: these contractions gradually disappear, before any changes from putrefaction are manifested; but the cup in the coagulum of blood does not relax in the same manner; hence it may be inferred, that the final contraction of muscles is not the coagulation of the blood contained in them; neither is it a change in the reticular membrane, nor in the blood-vessels, because such contractions are not general throughout those substances. The coagulation of the blood is a certain criterion of death. The reiterated visitations of blood are not essential to muscular irritability, because the limbs of animals, separated from the body, continue for a long time afterwards capable of contractions, and relaxations.

The constituent elementary materials of which the peculiar animal and vegetable substances consist, are not separable by any chemical processes hitherto instituted, in such manner as to allow of a recombination into their former state. The composition of these substances appears to be naturally of transient duration, and the attractions of the elementary materials which form the gross substances, are so loose and unsettled, that they are all decomposed without the intervention of any agent, merely by the operation of their own elementary parts on each other.

An extensive discussion of the chemical properties attaching to the matter of muscle would be a labour unsuited to this occasion; I should not, however, discharge my present duty, if I omitted to say, that all such investigations can only be profitable when effected by simple processes, and when made upon the raw materials of the animal fabric, such, perhaps, as the albumen of eggs, and the blood. But, until by synthetical experiments the peculiar substances of animals are composed from what are considered to be elementary materials, or the changes of organic secretion imitated by art, it cannot be hoped that any determinate knowledge should be established upon which the physiology of muscles may be explained. Such researches and investigations promise, however, the most probable ultimate success, since the phenomena are nearest allied to those of chemistry, and since all other hypotheses have, in their turns, proved unsatisfactory.

Facts and Experiments tending to support and illustrate the preceding Argument.

An emaciated horse was killed by dividing the medulla spinalis, and the large blood-vessels under the first bone of the sternum.

The temperature of the flowing blood was 103°
Spleen - - 103
Stomach - - 101
Colon - - 98
Bladder of urine 97
Atmosphere - 30.

Three pigs, killed by a blow on the head, and by the immediate division of the large arteries and veins, entering the middle of the basis of the heart, had the blood flowing from these vessels of 106, 106½, and 107°; the atmospheric temperature being at 31°.

An ox, killed in a similar manner, the blood 103°; atmosphere 50°.

Three sheep, killed by dividing the carotid arteries, and internal jugular veins: their blood 105, 105, 105½°; atmosphere 41°.

Three frogs, kept for many days in an equable atmosphere at 54°; their stomachs 62°.

The watery fluid issuing from a person tapped for dropsy of the belly 101°: the atmosphere being 43°, and the temperature of the superficies of the body at 96°.

These temperatures are considerably higher than the common estimation.

A man's arm being introduced within a glass cylinder, it was duly closed at the end which embraced the head of the humerus; the vessel being inverted, water at 97° was poured in, so as to fill it. A ground brass plate closed the lower aperture, and a barometer tube communicated with the water at the bottom of the cylinder. This apparatus including the arm, was again inverted, so that the barometer tube became a gage, and no air was suffered to remain in the apparatus. On the slightest action with the muscles of the hand, or fore-arm, the water ascended rapidly in the gage, making librations of six and eight inches length in the barometer tube, on each contraction and relaxation of the muscles.

The remarkable effects of crimping fish by immersion in water, after the usual signs of life have disappeared, are worthy attention; and whenever the rigid contractions of death have not taken place, this process may be practised with success. The sea fish destined for crimping are usually struck on the head when caught, which, it is said, protracts the term of this capability; and the muscles which retain this property longest are those about the head. Many transverse sections of the muscles being made, and the fish immersed in cold water, the contractions called crimping take place in about five minutes; but, if the mass be large, it often requires thirty minutes to complete the process.

Two flounders, each weighing 1926 grains, the one being in a state for crimping, the other dead and rigid, were put into water at 48°, each being equally scored with a knife. After half an hour, the crimped fish had gained in weight 53 grains, but the dead fish had lost 7 grains. The specific gravity of the crimped fish was greater than that of the dead fish, but a quantity of air-bubbles adhered to the surfaces of the crimped muscles, which were rubbed off before weighing; this gas was not inflammable.

The specific gravity of the crimped fish - - 1,105
of the dead fish, after an equal
immersion in water - 1,090.

So that the accession of water specifically lighter than the muscle of fish, did not diminish the specific gravity of crimped muscle, but the contrary: a proof that condensation had taken

A piece of cod-fish weighing twelve pounds, gained in weight, by crimping, two ounces avoirdupois; and another less vivacious piece, of fifteen pounds, gained one ounce and half.*

The hinder limb of a frog, having the skin stripped off, and weighing $77\frac{1}{10}$ grains, was immersed in water at 54°, and suffered to remain nineteen hours, when it had become rigid, and weighed $100\frac{1}{4}$ grains. The specific gravity of the contracted limb had increased, as in the crimped fish.

Six hundred and thirty grains weight of the subscapularis muscle of a calf, which had been killed two days from the 10th of January, was immersed in New River water at 45°. After ninety minutes, the muscle was contracted, and weighed in air 770 grains: it had also increased in specific gravity, but the quantity of air-bubbles formed in the intersticial spaces of the reticular membrane made it difficult to ascertain the degree.

Some of the smallest fasciculi of muscular fibres from the same veal, which had not been immersed in water, were placed on a glass plate, in the field of a powerful microscope, and a drop of water thrown over them, at the temperature of 54°, the atmosphere in the room being 57°. They instantly began to contract, and became tortuous.

On confining the ends of another fibril with little weights of glass, it contracted two-thirds of its former length, by similar treatment. The same experiment was made on the muscular fibres of lamb and beef, twelve hours after the animals had been killed, with the like results. Neither vinegar, nor water saturated with muriate of soda, nor strong ardent spirit, nor olive oil, had any such effect upon the muscular fibres.

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* I am informed that the crimping of fresh water fishes requires hard water, or such as does not suit the purposes of washing with soap. This fact is substantiated by the practice of the London fishmongers, whose experience has taught them to employ pump water, or what is commonly called hard water.

The amphibia, and coleopterous insects, become torpid at 34°. At 36° they move slowly, and with difficulty; and, at a lower temperature their muscles cease to be irritable. The muscles of warm-blooded animals are similarly affected by cold.

The hinder limbs of a frog were skinned and exposed to cold at 30°, and the muscles were kept frozen for eight hours, but on thawing them, they were perfectly irritable.

The same process was employed in the temperature of 20°, and the muscles kept frozen for twelve hours, but that did not destroy the irritability.

In the heat of 100°, the muscles of cold-blooded animals fall into the contractions of death; and at 110°, all those of warm blood, as far as these experiments have been extended. The muscles of warm-blooded animals, which always contain more red particles in their substance than those of cold blood, are sooner deprived of their irritability, even although their relative temperatures are preserved; and respiration in the former tribe is more essential to life than in the latter.

Many substances accelerate the cessation of irritability in muscles when applied to their naked fibrils, such as all the narcotic vegetable poisons, muriate of soda, and the bile of animals; but they do not produce any other apparent change in muscles, than that of the last contraction. Discharges of electricity passed through muscles, destroy their irritability, but leave them apparently inflated with small bubbles of gas;

perhaps some combination obtains which decomposes the water.

The four separated limbs of a recent frog were skinned, and immersed in different fluids; *viz.* No. 1, in a phial containing six ounces by measure of a saturated aqueous solution of liver of sulphur made with potash; No. 2, in a diluted acetic acid, consisting of one drachm of concentrated acid to six of water; No. 3, in a diluted alkali, composed of caustic vegetable alkali one drachm, of water six ounces; No. 4, in pure distilled water.

The phials were all corked, and the temperature of their contents was 46°.

The limb contained in the phial No. 1, after remaining twenty minutes, had acquired a pale red colour, and the muscles were highly irritable.

The limb in No. 2, after the same duration, had become rigid, white, and swollen; it was not at all irritable. By removing the limb into a diluted solution of vegetable alkali, the muscles were relaxed, but no signs of irritability returned.

No. 3, under all the former circumstances, retained its previous appearances, and was irritable, but less so than No. 1.

No. 4 had become rigid, and the final contraction had taken place.

Other causes of the loss of muscular irritability occur in pathological testimonies, some examples of which may not be ineligible for the present subject. Workmen whose hands are unavoidably exposed to the contact of white lead, are liable to what is called a palsy in the hands and wrists, from a torpidity of the muscles of the fore arm. This affection seems to be decidedly local, because, in many instances, neither the brain, nor the other members, partake of the disorder; and it oftenest affects the right hand. An ingenious practical chemist in London has frequently experienced spasms and rigidity in the muscles of his fore arms, from affusions of nitric acid over the cuticle of the hand and arm. The use of mercury occasionally brings on a similar rigidity in the masseter muscles.

A smaller quantity of blood flows through a muscle during the state of contraction, than during the quiescent state, as is evinced by the pale colour of red muscles when contracted. The retardation of the flow of blood from the veins of the fore arm, during venæsection, when the muscles of the limb are kept rigid, and the increased flow after alternate relaxations, induces the probability, that a temporary retardation of the blood in the muscular fibrils takes place during each contraction, and that its free course obtains again during the relaxation. This state of the vascular system in a contracted muscle, does not, however, explain the diminution of its bulk, although it may have some influence on the limb of a living animal.

When muscles are vigorously contracted, their sensibility to pain is nearly destroyed; this means is employed by jugglers for the purpose of suffering pins to be thrust into the calf of the leg, and other muscular parts with impunity: it is indeed reasonable to expect, *a priori*, that the sensation, and the voluntary influence, cannot pass along the nerves at the same time.

In addition to the influences already enumerated, the human muscles are susceptible of changes from extraordinary occurrences of sensible impressions. Long continued attention to interesting visible objects, or to audible sensations, are known to exhaust the muscular strength: intense thought and anxiety, weaken the muscular powers, and the passions of grief and fear produce the same effect suddenly: whilst the contrary feelings, such as the prospect of immediate enjoyment, or moderate hilarity, give more than ordinary vigour.

It is a very remarkable fact in the history of animal nature, that the mental operations may become almost automatic, and, under such habit, be kept in action, without any interval of rest, far beyond the time which the ordinary state of health permits, as in the examples of certain maniacs, who are enabled without any inconvenience, to exert both mind and body for many days incessantly. The habits of particular modes of labour and exercise are soon acquired, after which, the actions become automatic, demand little attention, cease to be irksome, and are effected with little fatigue: by this happy provision of nature, the habit of industry becomes a source of pleasure, and the same appears to be extended to the docile animals which co-operate with man in his labours.

Three classes of muscles are found in the more complicated animals. Those which are constantly governed by the will, or directing power of the mind, are called voluntary muscles. Another class, which operate without the consciousness of the mind, are denominated involuntary; and a mixed kind occur in the example of respiratory muscles, which are governed by the will to a limited extent; nevertheless the exigencies of the animal feelings eventually urge the respiratory movements in despight of the will. These last muscles appear to have become automatic by the continuance of habit.

The uses of voluntary muscles are attained by experience, imitation, and instruction: but some of them are never called into action among Europeans, as the muscles of the external ears, and generally the occipito-frontalis. The purely involuntary muscles are each acted upon by different substances, which appear to be their peculiar stimuli; and these stimuli co-operate with the sensorial influence in producing their contractions: for example, the bile appears to be the appropriate stimulus of the muscular fibres of the alimentary canal below the stomach, because the absence of it renders those passages torpid. The digested aliment, or perhaps the gastric juice in a certain state, excites the stomach. The blood stimulates the heart, light the iris of the eye, and mechanical pressure seems to excite the muscles of the œsophagus. The last cause may perhaps be illustrated by the instances of compression upon the voluntary muscles, when partially contracted, of which there are many familiar examples. Probably the muscles of the ossicula auditûs are awakened by the tremors of sound; and this may be the occasion of the peculiar arrangement observable in the chorda tympani, which serves those muscles.

These extraneous stimuli seem only to act in conjunction with the sensorial power, derived by those muscles from the gangliated nerves, because the passions of the mind alter the muscular actions of the heart, the alimentary canal, the respiratory muscles, and the iris; so that probably the respective stimuli already enumerated, only act subserviently, by awakening the attention of the sensorial power, (if that expression may be allowed,) and thereby calling forth the nervous influence, which, from the peculiar organization of the great chain of sympathetic nerves, is effected without consciousness: for, when the attention of the mind, or the more interesting passions prevail, all the involuntary muscles act irregularly, and unsteadily, or wholly cease. The movements of the iris of the common parrot is a striking example of the mixed influence.

The muscles of the lower tribes of animals, which are often entirely supplied by nerves coming from ganglions, appear of this class; and thus the animal motions are principally regulated by the external stimuli, of which the occurrence seems to agree with the animal necessities: but the extensive illustrations which comparative anatomy affords on this point, are much too copious for any detail in this place.

There are two states of muscles, one active, which is that of contraction, the other, a state of ordinary tone, or relaxation, which may be considered passive, as far as it relates to the mind; but the sensorial or nervous power seems never to be quiescent, as it respects either the voluntary or involuntary muscles during life. The yielding of the sphincters appears to depend on their being overpowered by antagonist muscles, rather than on voluntary relaxation, as is commonly supposed.

I have now finished this endeavour to exhibit the more recent historical facts connected with muscular motion.

It will be obvious to every one, that much remains to be done, before any adequate theory can be proposed. I have borrowed from the labours of others, without acknowledgement, because it would be tedious to trace every fact, and every opinion to its proper authority: many of the views are perhaps peculiar to myself, and I have adduced many general assumptions and conclusions, without offering the particular evidence for their confirmation, from a desire to keep in view the remembrance of retrospective accounts, and to combine them with intimations for future research. The due cultivation of this interesting pursuit cannot fail to elucidate many of the phenomena in question, to remove premature and ill founded physiological opinions, and eventually to aid in rendering the medical art more beneficial, by establishing its doctrines on more extensive and accurate views of the animal economy.

II. *Experiments for ascertaining how far Telescopes will enable us to determine very small Angles, and to distinguish the real from the spurious Diameters of celestial and terrestrial Objects: with an Application of the Result of these Experiments to a Series of Observations on the Nature and Magnitude of Mr.* Harding's *lately discovered Star. By* William Herschel, *LL.D. F.R.S.*

Read December 6, 1804.

THE discovery of Mr. HARDING having added a moving celestial body to the list of those that were known before, I was desirous of ascertaining its magnitude; and as in the observations which it was necessary to make I intended chiefly to use a ten-feet reflector, it appeared to me a desideratum highly worthy of investigation to determine how small a diameter of an object might be seen by this instrument. We know that a very thin line may be perceived, and that objects may be seen when they subtend a very small angle; but the case I wanted to determine relates to a visible disk, a round, well defined appearance, which we may without hesitation affirm to be circular, if not spherical.

In April of the year 1774, I determined a similar question relating to the natural eye: and found that a square area could not be distinguished from an equal circular one till the diameter of the latter came to subtend an angle of 2′ 17″. I did not think it right to apply the same conclusions to a telescopic view of an object, and therefore had recourse to the following experiments.

1st Experiment, with the Heads of Pins.

I selected a set of pins with round heads, and deprived them of their polish by tarnishing them in the flame of a candle. The diameters of the heads were measured by a microscopic projection, with a magnifying power of 80. These measures are so exact, that when repeated they will seldom differ more than a few ten thousandths parts of an inch from each other. Their sizes were as follows: ,1375 ,0863 ,0821 ,0602 ,0425. I placed the pins in a regular order upon a small post erected in my garden, at 2407,85 inches from the centre of the object mirror of my ten-feet reflecting telescope. The focal length of the mirror on Arcturus is 119,64 inches, but on these objects 125,9. The distance was measured with deal rods.

When I looked at these objects in the telescope, I found immediately that only the smallest of them, at this distance could be of any use; for with an eye-glass of 4 inches, which gives the telescope a magnifying power of no more than 31,5, this pin's head appeared to be a round body, and the view left no doubt upon the subject. It subtended an angle of 3″,64 at the centre of the mirror, and the magnified angle under which I saw it was 1′ 54″,6. This low power however required great attention.

With a lens 3,3, power 38,15, I saw it instantly round and globular. The magnified angle was 2′ 18″,9.

With a magnifying power of 231,8,* I saw it so plainly that the little notch in the pin's head between the coils of the wire making the head, appeared like a narrow black belt surrounding the pin in the manner of the belts of Jupiter. This notch by the microscopic projection measured ,00475 inch; and subtended an angle, at the centre of the mirror, of 0″,407.

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* The powers have been strictly ascertained as they are at the distance where these objects were viewed.

With 303,5 I saw the belt still better, and could follow it easily in its contour.

With 432,0 I could see down into the notch, and saw it well defined within.

With 522,3 the pin's head was a very striking globular object, whose diameter might easily be divided by estimation into ten parts, each of which would be equal to 0″,364.

With 925,6 I saw all the same phenomena still plainer.

The result of this experiment is, that an object having a diameter ,0425 may be easily seen in my telescope to be a round body, when the magnified angle under which it appears is 2′ 18″,9, and that with a high power a part of it, subtending an angle of 0″,364 may be conveniently perceived.

When I considered the purpose of this experiment, I found the result not sufficient to answer my intention; for as the size of the object I viewed obliged me to use a low power, a doubt arose whether the instrument would be equally distinct when a higher should be required. To resolve this question, it was necessary either to remove my objects to a greater distance, or to make them smaller.

2d Experiment, with small Globules of Sealing-wax.

I melted some sealing-wax thinly spread on a broad knife, and dipt the point of a fine needle, a little heated, into it, which took up a small globule. With some practice I soon acquired the art of making them perfectly round and extremely small. To prevent my seeing them at a distance in a different aspect from that in which they were measured under the microscope, I fixed the needles with sealing-wax on small slips of cards before the measures were taken.

Eight of these globules of the following dimensions ,0466 ,0325 ,0290 ,02194 ,0210 ,0169 ,0144 ,00763 were placed upon the post in my garden, and I viewed them in the telescope.

With a power of 231,8 I saw all the first seven numbers well defined, and round, and could see their gradual decrease very precisely from No. 1 to No. 7.

With 303,5 I saw them better, and had a glimpse of No. 8, but could not be sure that I saw it distinctly round; though the magnified angle was 3′ 18″,2.

With 432,0 they are all very palpable objects, and, as a solid body, No. 8 may be seen without difficulty; at the centre of the mirror it subtends an angle of 0″,653. With attention we may also be sure of its roundness; but here the magnified angle is not less than 4′ 42″,1.

With 522,3 I see them all in great perfection as spherical bodies, and the magnitude of No. 7 may be estimated in quarters of its diameters. The angle is 1″,253, and one quarter of it is 0″,313. No. 8 may be divided into two halves with ease; each of which is 0″,327.

With 925,4 I saw No. 8 still better; but sealing-wax is not bright enough for so high a power.

By this experiment it appears, that with a globule so small as ,00763 of a substance not reflecting much light, the magnified angle must be between 4 and 5 minutes before we can see it round. But it also appears that a telescope with a sufficient power, will show the disk of a faint object when the angle it subtends at the naked eye is no more than 0″,653.

3d Experiment, with Globules of Silver.

As the objects made of sealing-wax, on account of their colour, did not appear to be fairly selected for these investigations, I made a set of silver ones. They were formed by running the end of silver wires, the 305th and 340th part of an inch in diameter, into the flame of a candle. It requires some practice to get them globular, as they are very apt to assume the shape of a pear; but they are so easily made that we have only to reject those which do not succeed.

Thirteen of them, in a pretty regular succession of magnitude, were selected and placed upon the post. Their dimensions were ,03956 ,0371 ,0329 ,0317 ,0272 ,0260 ,0187 ,0178 ,0164 ,0125 ,01137 ,00800 ,00556.

For the sake of more conveniency I had removed my telescope from its station in the library to a work-room. The distance of the objects from the mirror of the telescope, measured with deal rods, was here only 2370,5 inches; and the focal length of the mirror, the magnifying powers of the telescope, and the angles subtended by the objects have been calculated accordingly.

With 522,7 I see all the globules, from No. 1 to No. 13, perfectly well, and can estimate the latter in quarters of its diameter. The angle it subtends at the centre of the mirror is 0″,484; and one quarter of it is 0″,121.

With the same power I see the wires which hold the balls, so well that even the smallest of them may be divided into half its thickness. It measures ,00237; the angle is 0″,206; and half of it 0″,103.

With 433,0 I see all the globules of a round form, and can by estimation divide No. 13 into two halves. The magnified angle is here 3′ 29″,0, but as its diameter could by estimation be divided into two parts, the round form of a globule somewhat less might probably have been perceived, so that the magnified angle would perhaps not have much exceeded the quantity 2′ 18″,9 that has been assigned before.

After some time the weather became much overcast, and as the globules were placed over a cut hedge, the leaves and interstices of which did not reflect much light, they received the greatest part of their illumination from above. This made them gradually assume the shape of half moons placed horizontally. The dark part of these little lunes, however, did not appear sensibly less than the enlightened part, so that there could not be any thing spurious about them.

By this experiment we find that the telescope acts very well with a high power, and will show an object subtending only 0″,484 so large that we may divide it into quarters of its diameter.

4th Experiment, with Globules of Pitch, Bee's-wax, and Brimstone.

I had before objected to sealing-wax globules on account of their dingy-red colour; in the last experiment a doubt was raised with regard to the silver ones, because they were perhaps too glossy. In order to compare the effect of different substances together in the same atmosphere, I put up three globules, No. 1 of silver, diameter ,01137; No. 2 of sealing-wax ,01125; No. 3 of pitch ,00653.

With 522,7 I saw No. 1 round, and could estimate ¼ of its diameter. The angle is 0″,989; ¼ of it is 0″,247.

I saw No. 2 round, but of a dusky-red colour. It is not nearly so bright as No. 1; nor does it appear quite so large as the proportional measure of the globules would require. I can estimate ⅓ of its diameter. The angle is 0″,979; and ⅓ of it is 0″,326.

No 3 reflects so little light that I can barely perceive the globule, but not its form; and yet it subtends an angle of 0″,568.

To discover whether this ought to be ascribed intirely to the want of reflection of the pitch, I took up some white melted bee's-wax, by dipping the fine point of a needle perpendicularly into it. This happened to be only half a globule, and its diameter was ,0105.

When I examined the object with 523 I saw it with great ease, and could estimate ¼ of its diameter. The angle is 0″,914; and ¼ of it is 0″,228. I saw also that it was but half a globule.

I took up another, that I might have a round one; but found that again I had only half a globule. It was so perfectly bisected, that art and care united could not have done it better. Its diameter was ,0108. In the telescope I saw its semiglobular form, and could estimate ¼ of its diameter.

By some further trials it appeared, that a perfect globule of this substance could not be taken up, the reason of which it is not difficult to perceive; for as it melts with very little heat, it will cool the moment the needle is lifted up; and the surface, which cools first, will be flat.

The roundness of the objects being a material circumstance, I melted a small quantity of the powder of brimstone, and dipping the point of a needle into it, I found that globules, perfectly spherical and extremely small, might be taken up. I had one of them that did not exceed the 640th part of an inch in diameter.

When four of the following sizes, ,00962 ,009125 ,00475 ,002375 were placed on the post in the garden and viewed from the work-room station with 522,7 I saw No. 1, 2, and 3, round, but No. 4 was invisible.

These globules reflect but little light, so that they are not easily to be distinguished from the surrounding illumination of the atmosphere; but when I placed some dark blue paper a few inches behind them, I then could also perceive No. 4 as a round body. The angle it subtends is 0",207.

5th Experiment with Objects at a greater Distance.

Having carried the minuteness of the globules as far as appeared to be proper, I considered that a valuable advantage would be gained by increasing the distance of the objects. The experiments might here be made upon a larger scale, and the body of air through which it would be necessary to view the globules would bring the action of the telescope more upon a par with an application of it to celestial objects.

On a tree, at 9620,4 inches from the object mirror of the telescope, I fixed the sealing-wax globules of the 2d experiment. The distance was measured by a chain compared with deal rods, and by calculation the altitude of the objects has been properly taken into the account.

With 502,6 No. 1 is a very large object; so that were I to see a celestial body under the same angle, I could never mistake it for a small star. The angle it subtends is 0″,999.

I see the diameters of No. 2 and 3 very clearly, and can divide them by estimation into two parts, half of No. 3 is 0″,311.

I see No. 4 and 5 as round bodies, but cannot divide them by estimation. The diameter of No. 5 is 0″,45. No. 6 may also be seen, but 7 and 8 are invisible.

These objects reflecting too little light, the silver globules of the 3d experiment were placed on the tree. It will be right to mention that they were all so far tarnished by having been out in the open air for more than a fortnight, that no improper reflection was to be apprehended.

The air being uncommonly clear, I saw with 502,6 the globules No. 1, 2, 3, 4, 5, and 6, as well defined black balls. I could easily distinguish $\frac{1}{4}$ of the diameter of No. 6; which is 0″,139.

With 415,7 I saw them all round as far as No. 10 included.

With 502,6 I saw No. 9 and 10 very sharp and black, and could divide No. 10 into two parts, each of which would be 0″,134.

With a new lens, power 759,7, I saw No. 10 better than with 502,6, and could with ease distinguish it into halves, or even third parts of its diameter. $\frac{1}{3}$ of it is 0″,089.

With 223,1 I saw them all as far as No. 10 included as visible objects, but the smallest of them were mere points. No. 6 might be divided with this power into two parts; each being 0″,279.

With 292,1 I saw No. 10 sharp and round. The magnified angle is only 1′ 18″,3. One half of No. 6 may be perceived with great ease.

The weather being as favourable as possible, I saw with 415,7 the globule No. 10 round at first sight; the magnified angle is 1′ 51″,2. I can see No. 12 steadily round; the angle is 0″,172. It is however a mere point, and divisions of it cannot be made.

With a new 10-feet reflector, power 540, the globule No. 10 is beautifully well defined, and $\frac{1}{2}$ of it may be estimated; the angle is 0″,268; $\frac{1}{2}$ of it is 0″,134.

With the old reflector, and 502,6, I see No. 12 steadily round. No. 7, 11, and 13, have met with an accident, and could not be observed.

6th Experiment with illuminated Globules.

The night being very dark, 8 silver globules, from ,0291 to ,00596 in diameter were placed on the post, and illuminated by a lantern held up against them.

With 522,7 I saw them all perfectly well, but the small quantity of light thrown on them was not sufficient to make angular experiments upon them. As objects I saw them as easily as in the day time. Probably the phases of the illuminated disks I saw might be such as the moon would show when about 9 or 10 days old. The angle of No. 8, had it been full, would have been 0″,519. A better way of illumination might be contrived.

SPURIOUS DIAMETERS OF CELESTIAL OBJECTS.

Observations and Experiments, with Remarks.

July 17, 1779. With a 7-feet reflector, power 280, I saw the body of Arcturus, very round and well defined. I saw also ζ Ursæ majoris and other stars equally round, and as well defined.

REMARKS.

(1.) As these diameters are undoubtedly spurious, it follows that, with the stars, the spurious diameters are larger than the real ones, which are too small to be seen.

Sept. 9, 1779. The two stars of ε Bootis are of unequal diameters; one of them being about three times as large as the other.

(2.) From this and many estimations of the spurious diameters of the stars* it follows, not only that they are of different sizes, but also that under the same circumstances, their dimensions are of a permanent nature.

August 25, 1780. The large star of γ Andromedæ is of a very fine reddish colour, and the small one blue.

(3.) By this and many other observations it appears, that the spurious diameters of the stars are differently coloured, and that these colours are permanent when circumstances are the same.

Nov. 23, 1779. I viewed α Geminorum with a power of 449, and saw the two stars in the utmost perfection. The vacancy between them was about 1½ diameter of the largest. I found when I looked with a lower power, that the proportion between the distance and magnitude of the stars underwent an alteration. With 222, the vacancy was 1¼ diameter, and with 112, it was no more than 1 diameter of the smallest of the two stars, or less.

(4.) By many observations, a number of instances of which may be seen in my catalogues of double stars, their spurious diameters are lessened by increasing the magnifying power, and increase when the power is lowered.

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* See Catalogues of double Stars. Phil. Trans. for 1782, p. 115; and for 1785, page 40.

(5.) It is also proved by the same observations, that the increase and decrease of the spurious diameters, is not inversely as the increase and decrease of the magnifying power, but in a much less ratio.

Nov. 13, 1782. The two stars of the double star 40 Lyncis, with a power of 460 are very unequal; and with 227 they are extremely unequal.

(6.) From this we find, that the magnifying power acts unequally on spurious diameters of different magnitudes; less on the large diameters, and more on the small ones.

Aug. 20, 1781. I saw ε Bootis with 460, and the vacancy between the two stars was 1¼ diameter of the large one. I then reduced the aperture of the telescope by a circle of pasteboard from 6,3 inches to 3,5, and the vacancy between the two stars became only ½ diameter of the small star.

The proportion of the diameters of the two stars to each other was also changed considerably; for the small one was now at least ⅔ if not ¾ of the large one.

(7.) This shows that when the aperture of the telescope is lessened, it will occasion an increase of the spurious diameters, and when increased will reduce them.

(8.) It also shows that the increase and decrease of the unequal spurious diameters, by an alteration of the aperture of the telescope, is not proportional to the diameters of the stars:

(9.) But that this alteration acts more upon small spurious diameters, and less upon large ones.

Aug. 7, 1783. I tried some excessively small stars near γ

Aquilæ. When γ was perfectly distinct and round, the extremely small stars were dusky and ill defined; the *excessively* small ones were still less defined. As there are stars of all sizes in this neighbourhood, I saw some so very minute, that they only had the appearance of a small dusky spot, approaching to mere nebulosity. By very long attention I perceived many small dusky nebulous spots, which had it not been for this attention might have been in the field of view without the least suspicion.

(10.) From this we find that stars, when they are extremely small, lose their spurious diameters, and become nebulous.

July 7, 1780. I saw the spurious diameter of Arcturus gradually diminished by a haziness of the atmosphere till it vanished intirely.

A more circumstantial account of this observation has already been given; and some other causes that affect the spurious diameter of the stars, have been pointed out in the same paper, such as tremulous air, wind, and hoar-frost.*

January 31, 1783. The star in the back of Columba makes a spectrum, about 5 or 6″ long, and about 2″ broad, finely coloured by the prismatic power of the atmosphere at this altitude.

July 28, 1783. Fomalhaut gives a beautiful prismatic spectrum, on account of its low situation.

July 17, 1781. With a new lens, power between 5 and 6 hundred, I saw ζ Aquarii, and found the vacancy between the two stars exactly 2 diameters. With my old one, power only 460, it was full 2 diameters. As it should have been larger with the high power than with the low one, it shows that the best eye-lens will give the least spurious diameter.

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* See Phil. Trans. for 1803, page 224.

Oct. 12, 1782. I tried a new plain speculum, made by a very good workman, and found that when I viewed α Geminorum with 460, the vacancy between the two stars was barely 1½ diameter, but the same telescope and power with my own small speculum, made the distance 2 diameters, so that the figure of this mirror affects the spurious diameters of the stars.

(11.) Hence we may conclude that many causes will have an influence on the apparent diameter of the spurious disks of the stars; but they are so far within the reach of our knowledge, that with a proper regard to them, the conclusion we have drawn in Rem. (2.) "that under the same circumstances "their dimensions are permanent," will still remain good.

SPURIOUS DIAMETERS OF TERRESTRIAL OBJECTS, WITH SIMILAR REMARKS.

7th Experiment with Silver Globules.

A number of silver globules were put on the post, before they had been tarnished; and the sun shone upon them. When I viewed them in the telescope, there was on each of them a lucid appearance resembling the spurious disk of a star. I could distinguish this bright spot from the real diameters of the globules perfectly well, and found it much less than they were.

REM. (1.) Hence we conclude that the terrestrial, spurious disks of globules are less than the real disks; whereas we have seen, in Remark (1.) of the celestial spurious disks, that these are larger than the real ones.

8th Experiment.

The luminous spots, or spurious disks of the globules were of unequal diameters. The globule No. 1 had the largest disk, and the smaller ones the least; and the gradation of the sizes followed the order of the numbers.

(2.) This agrees with the spurious disks of celestial objects: the stars of the first, second, and third magnitude having a larger spurious disk than those that are of inferior magnitudes.

9th Experiment.

I found that there was a considerable difference in the colour of the spurious disks; one of them was of a beautiful purple colour, another was inclined to orange, a large one was straw coloured, a small one pale-ash coloured, and most of them were bluish-white.

(3.) With respect to colours, therefore, the terrestrial also agree with the celestial spurious disks.

10th Experiment.

I made two globules of different diameters, and placed them very near each other, so that their spurious disks might resemble those of a double star; this succeeded perfectly well. I viewed them with different powers.

With 177, the vacancy between them is ¾ diameter of the large star.

With 232, it is 1¼ diameter.

With 303,8, it is 1⅝ diameter.

With 432,3, it is 1¾.

(4.) This experiment proves that the spurious diameters of the globules are also in this respect like the spurious disks of the stars; for they are proportionally lessened by increasing the magnifying power, and increased when the power is lowered.

(5.) When the estimations are compared with the powers, it will also be seen that the increase and decrease of the spurious disks of the globules is not inversely as the powers, but in a much less ratio.

11*th Experiment.*

Two other globules of different sizes were examined; and

With 706,3 they were pretty unequal.

With 522,7 they were considerably unequal.

With 303,8 they were very unequal.

(6.) This proves that the effect of magnifying power is unequally exerted on spurious diameters; and that, as with celestial objects, so with terrestrial, this power acts more on the small spurious disks than on the large ones.

12*th Experiment.*

I viewed a different artificial double star with 522,7, and keeping always the same power, changed the aperture of the telescope.

With the inside rays I found them considerably unequal, and 2½ diameters of the largest asunder. The spurious disks are perfectly well defined, round, and of a planetary aspect.

With all the mirror open, they are also round and well defined.

With the outside rays, they are near 4 diameters of the largest asunder, and are also round and distinct, but surrounded with flashing rays and bright nodules in continual motion.

(7.) This shows that the spurious terrestrial disks, in this respect again resemble those of the stars; increasing when the aperture is lessened, and decreasing when it is enlarged.

13th *Experiment.*

With the same magnifying power 432,3, but a change of aperture, I viewed two equal globules, and two unequal ones.

With the inside rays the equal globules were 1 diameter asunder.

With all the mirror open, they were 1½ diameter asunder.

And with the outside rays they were 2 diameters asunder.

The unequal globules, with the inside rays, were a little unequal, and 1 diameter of the large one asunder.

With the outside rays they were considerably unequal, and 2 diameters of the large one asunder.

(8.) By these experiments it is proved, that the increase and decrease of the diameters occasioned by different apertures is not proportional to the diameters of the spurious disks.

(9.) But that the change of the apertures acts more on the small, and less on the large ones.

14th *Experiment.*

No. 1 of a set of globules, has the largest spurious diameter. No. 3 is larger than No. 2; whereas No. 2 has the largest real diameter. It is inclined to a greenish colour. No. 3 is now reddish, and is larger than No. 1, which is at present less than No. 2. No. 1 grows bigger, and is now the largest.

The sun which had been shining, was obscured by some clouds, but the spurious diameters of the globules I was viewing remained visible, and were almost as bright as when the sun shone upon them.

I saw one of the globules lose its spurious diameter while the sun continued to shine. After some time the spurious diameter came on again, and very gradually grew brighter, but not larger. The colour of one of the globules being of a beautiful purple, changed soon after to a brilliant white.

The sun being obscured by some clouds, a globule lost its spurious diameter, and acquired the shape of an half moon, of the size of the real disk or diameter of the globule. I saw the sun break out again, and the half moon was gradually transformed into a much smaller spurious disk.

(10.) The spurious disks of globules are lost for want of proper illumination, but do not change their magnitude on that account. The brightness of the atmosphere in a fine day is sufficient to produce them; though the illumination of the sun is generally the principal cause of them.

(11.) The diameters of spurious disks are liable to change from various causes; an alteration in the direction of the illumination will make the reflection come from a different part of the globule, which can hardly be expected to be equally polished in its surface, or of equal convexity every where, being very seldom perfectly spherical; but as upon the whole the figure of them is pretty regular, the apparent diameter of the spurious disks will generally return to its former size.

15th *Experiment, with Drops of Quicksilver.*

At a time of the year when bright sun-shine is not very frequent, I found that my silver globules would seldom give me an opportunity for experiments on spurious disks; to obviate this inconvenience, I used small drops of quicksilver. They are more lucid, and will give a bright spot with very little sunshine. Many of these drops of all sizes were exposed upon a plate of glass, and some on slips of steel. The management of them is a little different from that of the globules. For in order to represent a double star these must be placed one almost behind the other, as otherwise they cannot be brought near enough without running together. The following general observation will include all the necessary particulars.

The bright spots on drops of quicksilver are very small compared to the size of the drops.

They are not proportional to the magnitude of the drops, though less on the small ones and greater on large ones.

In some of the large ones the bright spot is about $\frac{1}{30}$ or $\frac{1}{40}$ of the diameter of the drop.

The magnitude of the luminous spots is liable to changes, but is rather more permanent than with the silver globules.

There is a little difference in the colour of the luminous spots; they are generally of a brilliant white, but sometimes they incline to yellow, and the small ones to ash-colour.

With high magnifying powers they are very well defined, and, on account of their brightness, will bear these powers better than the silver globules.

If M and *m*, stand for the diameters of the large and small mirror of my telescope, then will an aperture $= \sqrt{\frac{M^2 - m^2}{2} + m^2}$ give half the light of the telescope. With this I examined two of the drops, and found the luminous spots upon them with

925,4 nearly equal, and 2½ diameters of the largest asunder.

706,3 nearly equal, and above 2 diameters of the largest asunder.

432,3 pretty unequal, and 2 diameters of the largest asunder.

177,0 considerably unequal, and 1¼ diameter of the largest asunder.

I examined also two other drops, with different apertures, without changing the power, which was 706,3.

With the inside rays they were very little unequal, and ¾ diameter of the largest asunder.

With the outside rays they were considerably unequal, and 1¼ diameter of the largest asunder.

From what has been said, it appears that all the remarks which have been made with regard to the spurious disks of the silver globules are confirmed by the luminous spots on the drops of quicksilver. There is a difference in the proportion which the spurious disks on quicksilver bear to the drops, and that on the silver globules to the size of the globules; the latter also give a greater variety of colours and magnitudes than those on quicksilver; these are circumstances of which it would be easy to assign the cause, but they can be of no consequence to the result we have drawn from the experiment.

16th *Experiment, with black and white Circles.*

I tried to measure some of the spurious disks by projecting them on a scale with a moveable index, but found their diameters were too small for accuracy by this method; for this reason I had recourse to artificial measuring-disks, and prepared a set of eleven white circles on a black ground, and eight black ones on a white ground. In order to guard against deceptions, I fixed them up against a tablet 154 inches from the eye, where it was intended to project the spurious disks of the globules, and examined them at that distance with the naked eye. Comparing then the size of the black to the white, I judged No. 1 of the black to be a little larger than No. 6 of the white circles. By a measure taken afterwards, it appeared that the black one was ,40 and the white ,39. Without supposing that every estimation may be made at this distance with equal accuracy, to the hundredth part of an inch, it is sufficiently evident that no material deception can take place in estimating by either of the sets of circles on account of their colour.

17*th Experiment, with different Illumination.*

A similar experiment was made in the microscope, by which the globules were measured. Two of them were placed on the measuring stand, and with an illumination from below, they appeared black, and were projected on white paper. The diameter of each globule and the distance between them were then measured. After this, I caused the illumination to come from above, and the globules being now of a silvery white, were projected on a slate. In this situation, when I repeated the former measures, no difference could be perceived.

18*th Experiment. Measures of spurious Disks.*

The spurious disk of a globule was then projected on the tablet where the white circles were placed. While I was comparing it with No. 4, which is ,31 in diameter and estimated it to be a little less than the circle, the spurious disk grew brighter; but it remained still of the same size; so that a variation in the quantity of the illumination will make no difference.

Every thing being now arranged for the measurement, I viewed the spurious diameter, with a magnifying power of 522,7, and compared it to the circles which succeeded each other by small differences of magnitude.

With all the mirror, from the centre to 8,8 inches open, the diameter of the spurious disk was ,31 inches.

With 6,3 inches open, it was less than ,40 and larger than ,355.

With 5 inches open, it was ,40.

With 4 inches open, it was ,42.

With 3 inches open, it was ,465 nearly.

From these measures it might be supposed that by lessening the quantity of light, we bring on a certain indistinctness which gives more diameter to the spurious object; to prove that this is not the cause of the increase, I used the following apertures.

With an annular opening from 6,5 to 8,8 inches, the spurious disk was rather less than ,18.

With another from 5 to 8,8 it was exactly ,18.

With an opening from 4 to 6,5 it was ,22.

With another from 1,6 to 4 it was ,42.

(12.) Now since the outside rim from 6,5 to 8,8, which reflected less than half the light of the mirror, produced a spurious disk less than ,18 in diameter, and the whole light as we have seen gave a disk of ,31, it is evidently not the quantity of the light, but the part of the mirror from which it is reflected, that we are to look upon as the cause of the magnitude of the spurious disks of objects.

(13.) These measures therefore point out an improvement in my former method of putting any terrestrial disk we suspect to be spurious to the test. For the inside rays of a mirror, as before, will increase the diameter of these disks, but the outside rays alone will have a greater effect in reducing it, than when the inside rays are left to join with them.

19th *Experiment. Trial of Estimations.*

I placed two silver globules at a small distance from each other upon the post, but without measuring either the globules or their distance. When I viewed them with 522,7 they appeared in the shape of two half moons in an horizontal situation. The unenlightened parts of them were also pretty distinctly visible. I estimated the vacancy between the cusps of the lunes to be $\frac{1}{4}$ diameter of the largest.

On measuring the diameters and distance under the microscope, it appeared that the largest was ,0312; a quarter of which is ,0078. The distance of the globules from each other measured ,0111. The difference in the estimation ,0033 is less than $\frac{1}{300}$ part of an inch.

The experiment was repeated with a change of the distance of the globules from each other. They were then estimated to be less than the diameter of the large one asunder, but full that of the small one. When they were measured it was found that their distance was ,02608, and the diameter of the small one was ,0247, which estimation is still more accurate than the former.

20th *Experiment. Use of the Criterion.*

It remained now to be ascertained whether these half moons were spurious or real; for although I could also imperfectly perceive the dark part of the disks of the globules, yet a doubt would arise whether the two halves were really of equal magnitude; to resolve this question, I viewed them first with the inside rays of the mirror, then with the outside, and found that in both cases the distance of the lunes remained without the least alteration. I viewed them also with the whole mirror open, but it occasioned no change.

21*st Experiment. Measures of the comparative Amount of the spurious Diameters, produced by the Inside and Outside Rays.*

I divided the aperture of the mirror into two parts, one from 0 to 4,4 and the other from 4,4 to 8,8 inches. When I measured the spurious diameter of a globule, the inside rays made it ,40; with all the mirror open it was ,31; and with the outside rays it was ,22.

(14.) From this we may conclude that the diameters given by the inside rays, by all the mirror open, and by the outside rays, are in an arithmetical progression; and that the inside rays will nearly double, the diameter given by the outside. It remains however to be ascertained whether this will hold good with spurious disks of various magnitudes.

It will not be necessary to carry the divisions of the aperture farther; for as the application of these experiments is chiefly intended for astronomical purposes, we can hardly do with less than half the mirror open; and on the other hand with a very narrow rim of reflection from the outside of the mirror, distinctness would be apt to fail.

22*d Experiment. Trial of the Criterion on celestial Objects.*

I viewed α Lyræ with the outside rays, and found its spurious disk to be small; with all the mirror open it was larger, and with the inside rays it was largest.

As far as the imagination will enable us to compare objects we see in succession, the magnitudes appeared to be in an arithmetical progression.

23d Experiment.

I examined α Geminorum with 410,5, and with the outside rays the stars were considerably unequal, and 1¼ diameter of the largest asunder. With all the mirror open they were more unequal, and 1½ diameter of the largest. With the inside rays they were very unequal, and 1⅞ of the largest asunder.

These experiments show that, if it had not been known that the apparent disks of the stars were spurious, the application of the improved criterion of the apertures would have discovered them to be so; and that consequently the same improvement is perfectly applicable to celestial objects.

OBSERVATIONS ON THE NATURE AND MAGNITUDE OF MR. HARDING'S LATELY DISCOVERED STAR.

It will be remembered that in a former Paper, where I investigated the nature of the two asteroids discovered by Signior Piazzi and Dr. Olbers, I suggested the probability that more of them would soon be found out; it may therefore be easily supposed that I was not much surprised when I was informed of Mr. Harding's valuable discovery.

On the day I received an account of it, which was the 24th of September, I directed my telescope to the calculated place of the new object, and noted all the small stars within a limited compass about it. They were then examined with a distinct high magnifying power; and since no difference in their appearance was perceivable, it became necessary to attend to the changes that might happen in the situation of any one of them. They were delineated as in Fig. 1, (Plate I.) which is a mere eye-draught, to serve as an elucidation to a description given with it in the journal; and the star marked *k*, as will be seen hereafter, was the new object.

Sept. 25. The moon was too bright to see minute objects well, and my description the night before, for the same reason, had not been sufficiently particular; nor did I expect, from the account received, that the star had retrograded so far in its orbit.

Sept. 26. The weather being very hazy, no regular observations could be made; but as I noticed very particularly a star not seen before, it was marked *l* in Fig. 2, and proved afterwards to have been the lately discovered one, though still unknown this evening, for want of fixed instruments.

Sept. 27. I was favoured with Dr. MASKELYNE'S account of the place of the star, taken at the Royal Observatory, by which communication I soon found out the object I was looking for.

Sept. 29. Being the first clear night, I began a regular series of observations; and as the power of determining small angles, and distinctness in showing minute disks, whether spurious or real, of the instrument I used on this occasion, has been sufficiently investigated by the foregoing experiments, there could be no difficulty in the observation, with resources that were then so well understood, and have now been so fully ascertained.

" Mr. HARDING'S new celestial body precedes the very
" small star in Fig. 3, between 29 and 33 Piscium, and is a
" little larger than that star; it is marked A. *f g h* are taken
" from Fig. 1. I suppose *g* to be of about the 9th magnitude,

" so that the new star may be called a small one of the
" 8th."

With the 10-feet reflector, power 496,3, I viewed it attentively, and comparing it with *g* and *h*, Fig. 3, could find no difference in the appearance but what might be owing to its being a larger star.

By way of putting this to a trial, I changed the power to 879,4, but could not find that it magnified the new one more than it did the stars *g* and *h*.

" I cannot perceive any disk; its apparent magnitude with
" this power is greater than that of the star *g*, and also a very
" little greater than that of *h*; but in the finder, and the night-
" glass *g* is considerably smaller than the new star, and *h* is
" also a very little smaller."

I compared it now with a star which in the finder appeared to be a very little larger; and in the telescope with 879,4 the apparent magnitude of this star was also larger than that of the new one.

" As far as I can judge without seeing the asteroids of Mr.
" PIAZZI and Dr. OLBERS at the same time with Mr. HARDING'S,
" the last must be at least as small as the smallest of the
" former, which is that of Dr. OLBERS."

" The star *k*, Fig. 1, observed Sept. 24, is wanting, and was
" therefore the object I was in search of, which by computation
" must have been that day in the place where I saw it."

" The new star being now in the meridian with all those to
" which I am comparing it, and the air at this altitude being
" very clear, I still find appearances as before described: the
" new object cannot be distinguished from the stars by mag-
" nifying power, so that this celestial body is a true ASTEROID."

Mr. BODE's stars 19, 25 and 27 Ceti are marked 7m, and by comparing the asteroid, which I find is to be called Juno, with these stars, it has the appearance of a small one of the 8th magnitude.

With regard to the diameter of Juno, which name it will at present be convenient to use, leaving it still to astronomers to adopt any other they may fix upon, it is evident that, had it been half a second, I must have instantly perceived a visible disk. Such a diameter, when I saw it magnified 879,4 times, would have appeared to me under an angle of 7′ 19″,7, one half of which, it will be allowed, from the experiments that have been detailed, could not have escaped my notice.

Oct. 1. Between flying clouds, I saw the asteroid, which in its true starry form has left the place where I saw it Sept. 29. It has taken the path in which by calculation I expected it would move. This ascertains that no mistake in the star was made when I observed it last.

Oct. 2, 7^h^. Mr. HARDING's asteroid is again removed, but is too low for high powers.

8^h^ 30′. I viewed it now with 220,3 288,4 410,5 496,3 and 879,4. No other disk was visible than that spurious one which such small stars have, and which is not proportionally magnified by power.

With 288,4, the asteroid had a larger spurious disk than a star which was a little less bright, and a smaller spurious disk than another star that was a little more bright.

Oct. 5, with 410,5. The situation of the asteroid is now as in Fig. 4. I compared its disk, which is probably the spurious appearance of stars of that magnitude, with a larger, an equal, and a smaller star. It is less than the spurious disk of the larger, equal to that of the equal, and larger than that of the smaller star. The gradual difference between the three stars is exceedingly small.

" With 496,3, and the air uncommonly pure and calm, I see
" so well that I am certain the disk, if it be not a spurious one,
" is less than one of the smallest globules I saw this morning
" in the tree."

The diameter of this globule was ,02. It subtended an angle of 0",429, and was of sealing-wax; had it been a silver one, it would have been still more visible.

With 879,4. All comparative magnitudes of the asteroid and stars, remain as with 496,3.

I see the minute double star *q* Ophiuchi * in high perfection, which proves that the air is clear, and the telescope in good order.

The asteroid being now in the meridian, and the air very pure, I think the comparative diameter is a little larger than that of an equal star, and its light also differs from star-light. Its apparent magnitude, however, can hardly be equal to that of the smallest globule I saw this morning. This globule measured ,01358, and at the distance of 9620,4 inches subtended an angle of 0",214.

When I viewed the asteroid with 879,4 I found more haziness than an equal star would have given: but this I ascribe to want of light. What I call an equal star, is one that in an achromatic finder appears of equal light.

Oct. 7. Mr. HARDING's asteroid has continued its retrograde motion. The weather is not clear enough to allow the use of high powers.

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* See Cat. of double Stars, I. 87.

Oct. 8. If the appearance resembling the spurious disks of small stars, which I see with 410,5 in Mr. HARDING's asteroid, should be a real diameter, its quantity then by estimation may amount to about 0″,3. This judgment is founded on the facility with which I can see two globules often viewed for this purpose.

The angle of the first is 0″,429, and of the other 0″,214; and the asteroid might be larger than the latter, but certainly was not equal to the former.

With 496,3, there is an ill defined hazy appearance, but nothing that may be called a disk visible. When there is a glimpse of more condensed light to be seen in the centre, it is so small that it must be less than two-tenths of a second.

To decide whether this apparent condensed light was a real or spurious disk, I applied different limitations to the aperture of the telescope, but found that the light of the new star was too feeble to permit the use of them. From this I concluded that an increase of light might now be of great use, and viewed the asteroid with a fine 10-feet mirror of 24 inches diameter, but found that nothing was gained by the change. The temperature indeed of these large mirrors is very seldom the same as that of the air in which they are to act, and till a perfect uniformity takes place, no high powers can be used.

The asteroid in the meridian, and the night beautiful. After many repeated comparisons of equal stars with the asteroid, I think it shows more of a disk than they do, but it is so small that it cannot amount to so much as 3-tenths of a second, or at least to no more.

It is accompanied with rather more nebulosity than stars of the same size.

The night is so clear, that I cannot suppose vision at this altitude to be less perfect on the stars, than it is on day objects at the distance of 800 feet in a direction almost horizontal.

Oct. 11. By comparing the asteroid alternately and often with equal stars, its disk, if it be a real one, cannot exceed 2, or at most 3-tenths of a second. This estimation is founded on the comparative readiness with which every fine day I have seen globules subtending such angles in the same telescope, and with the same magnifying power.

" The asteroid is in the meridian, and in high perfection. I " perceive a well defined disk that may amount to 2 or 3-tenths " of a second; but an equal star shows exactly the same ap- " pearance, and has a disk as well defined and as large as that " of the asteroid."

RESULT AND APPLICATION OF THE EXPERIMENTS AND OBSERVATIONS.

We may now proceed to draw a few very useful conclusions from the experiments that have been given, and apply them to the observations of the star discovered by Mr. HARDING; and also to the similar stars of Mr. PIAZZI and Dr. OLBERS. These kind of corollaries may be expressed as follows.

(1.) A 10-feet reflector will show the spurious or real disks, of celestial and terrestrial objects, when their diameter is $\frac{1}{4}$ of a second of a degree; and when every circumstance is favourable, such a diameter may be perceived so distinctly, that it can be divided by estimation into two or three parts.

(2.) A disk of $\frac{1}{4}$ of a second in diameter, whether spurious or real, in order to be seen as a round, well defined body, requires a distinct magnifying power of 5 or 6 hundred, and must be sufficiently bright to bear that power.

(3.) A real disk of half a second in diameter will become so much larger by the application of a magnifying power of 5 or 6 hundred, that it will be easily distinguished from an equal spurious one, the latter not being affected by power in the same proportion as the former.

(4.) The different effects of the inside and outside rays of a mirror, with regard to the appearance of a disk, are a criterion that will show whether it is real or spurious, provided its diameter is more than $\frac{1}{4}$ of a second.

(5.) When disks, either spurious or real, are less than $\frac{1}{4}$ of a second in diameter, they cannot be distinguished from each other; because the magnifying power will not be sufficient to make them appear round and well defined.

(6.) The same kind of experiments are applicable to telescopes of different sorts and sizes, but will give a different result for the quantity which has been stated at $\frac{1}{4}$ of a second of a degree. This will be more when the instrument is less perfect, and less when it is more so. It will also differ even with the same instrument, according to the clearness of the air, the condition, and adjustment of the mirrors, and the practical habits of the observer.

With regard to Mr. HARDING's new starry celestial body, we have shown, by observation, that it resembles, in every respect, the two other lately discovered ones of Mr. PIAZZI and Dr. OLBERS; so that Ceres, Pallas, and Juno, are certainly three individuals of the same species.

That they are beyond comparison smaller than any of the seven planets cannot be questioned, when a telescope that will show a diameter of $\frac{1}{4}$ of a second of a degree, leaves it undecided whether the disk we perceive is a real or a spurious one.

A distinct magnifying power, of more than 5 or 6 hundred, has been applied to Ceres, Pallas, and Juno, but has either left us in the dark, or at least has not fully removed every doubt upon this subject.

The criterion of the apertures of the mirror, on account of the smallness of these objects, has been as little successful; and every method we have tried has ended in proving their resemblance to small stars.

It will appear, that when I used the name asteroid to denote the condition of Ceres and Pallas, the definition I then gave of this term* will equally express the nature of Juno, which, by its similar situation between Mars and Jupiter, as well as by the smallness of its disk, added to the considerable inclination and excentricity of its orbit, departs from the general condition of planets. The propriety therefore of using the same appellation for the lately discovered celestial body cannot be doubted.

Had Juno presented us with a link of a chain, uniting it to those great bodies, whose rank in the solar system I have also defined,† by some approximation of a motion in the zodiac, or by a magnitude not very different from a planetary one, it might have been an inducement for us to suspend our judgment with respect to a classification; but the specific difference between planets and asteroids appears now by the addition of a third individual of the latter species to be more fully established, and that circumstance, in my opinion, has added more to the ornament of our system than the discovery of another planet could have done.

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* See Phil. Trans. for 1802, p. 229, line 10.

† Ibid. page 224, line 3 of the same Paper.

Slough, near Windsor,
Dec. 1, 1804.

Fig. 1.

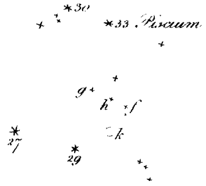


Fig. 2.

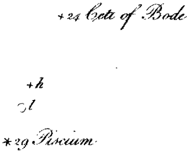


Fig. 3

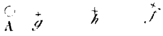
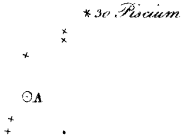


Fig. 4.



Basire f.

III. *An Essay on the Cohesion of Fluids.* *By* Thomas Young, *M. D. For. Sec. R. S.*

Read December 20, 1804.

I. *General Principles.*

It has already been asserted, by Mr. Monge and others, that the phenomena of capillary tubes are referable to the cohesive attraction of the superficial particles only of the fluids employed, and that the surfaces must consequently be formed into curves of the nature of lintearlæ, which are supposed to be the results of a uniform tension of a surface, resisting the pressure of a fluid, either uniform, or varying according to a given law. Segner, who appears to have been the first that maintained a similar opinion, has shown in what manner the principle may be deduced from the doctrine of attraction, but his demonstration is complicated, and not perfectly satisfactory; and in applying the law to the forms of drops, he has neglected to consider the very material effects of the double curvature, which is evidently the cause of the want of a perfect coincidence of some of his experiments with his theory. Since the time of Segner, little has been done in investigating accurately and in detail the various consequences of the principle.

It will perhaps be most agreeable to the experimental philosopher, although less consistent with the strict course of logical argument, to proceed in the first place to the comparison of this theory with the phenomena, and to inquire afterwards for its foundation in the ultimate properties of matter. But it is necessary to premise one observation, which appears to be new, and which is equally consistent with theory and with experiment; that is, that for each combination of a solid and a fluid, there is an appropriate angle of contact between the surfaces of the fluid, exposed to the air, and to the solid. This angle, for glass and water, and in all cases where a solid is perfectly wetted by a fluid, is evanescent: for glass and mercury, it is about 140°, in common temperatures, and when the mercury is moderately clean.

II. *Form of the Surface of a Fluid.*

It is well known, and it results immediately from the composition of forces, that where a line is equably distended, the force that it exerts, in a direction perpendicular to its own, is directly as its curvature; and the same is true of a surface of simple curvature; but where the curvature is double, each curvature has its appropriate effect, and the joint force must be as the sum of the curvatures in any two perpendicular directions. For this sum is equal, whatever pair of perpendicular directions may be employed, as is easily shown by calculating the versed sines of two equal arcs taken at right angles in the surface. Now when the surface of a fluid is convex externally, its tension is produced by the pressure of the particles of the fluid within it, arising from their own weight, or from that of the surrounding fluid; but when the surface is concave, the tension is employed in counteracting the pressure of the atmosphere, or, where the atmosphere is excluded, the equivalent pressure arising from the weight of the particles suspended from it by means of their cohesion, in the same manner as, when water is supported by the atmospheric pressure in an inverted vessel, the outside of the vessel sustains a hydrostatic pressure proportionate to the height; and this pressure must remain unaltered, when the water, having been sufficiently boiled, is made to retain its situation for a certain time by its cohesion only, in an exhausted receiver. When, therefore, the surface of the fluid is terminated by two right lines, and has only a simple curvature, the curvature must be every where as the ordinate; and where it has a double curvature, the sum of the curvatures in the different directions must be as the ordinate. In the first case, the curve may be constructed by approximation, if we divide the height at which it is either horizontal or vertical into a number of small portions, and taking the radius of each portion proportional to the reciprocal of the height of its middle point above or below the general surface of the fluid, go on to add portions of circles joining each other, until they have completed as much of the curve as is required. In the second case, it is only necessary to consider the curve derived from a circular basis, which is a solid of revolution; and the centre of that circle of curvature, which is perpendicular to the section formed by a plane passing through the axis, is in the axis itself, consequently in the point where the normal of the curve intersects the axis: we must therefore here make the sum of this curvature, and that of the generating curve, always proportional to the ordinate. This may be done mechanically, by beginning at the vertex, where the two curvatures are equal, then, for each succeeding portion, finding the radius of curvature by deducting the proper reciprocal of the normal, at the beginning of the portion, from the ordinate, and taking the reciprocal of the remainder. In this case the analysis leads to fluxional equations of the second order, which appear to afford no solution by means hitherto discovered; but the cases of simple curvature may be more easily subjected to calculation.

III. *Analysis of the simplest Forms.*

Supposing the curve to be described with an equable angular velocity, its fluxion, being directly as the radius of curvature, will be inversely as the ordinate, and the rectangle contained by the ordinate and the fluxion of the curve will be a constant quantity; but this rectangle is to the fluxion of the area, as the radius to the cosine of the angle formed by the curve with the horizon; and the fluxion of the area varying as the cosine, *the area* itself will vary as the sine of this angle, and *will be equal to the rectangle contained by the initial ordinate, and the sine corresponding to each point of the curve in the initial circle of curvature.* Hence it follows, first, that *the whole area included by the ordinates where the curve is vertical and where it is horizontal, is equal to the rectangle contained by the ordinate and the radius of curvature;* and, secondly, that the area on the convex side of the curve, between the vertical tangent and the least ordinate, is equal to the whole area on the concave side of the curve between the same tangent and the greatest ordinate.

In order to find the ordinate corresponding to a given angular direction, we must consider that the fluxion of the ordinate at the vertical part, is equal to the fluxion of the circle of curvature there, that, in other places, it varies as the radius of curvature and the sine of the angle formed with the horizon conjointly, or as the ordinate inversely, and directly as the sine of elevation; therefore the fluxion of the ordinate multiplied by the ordinate is equal to the fluxion of any circle of curvature multiplied by its corresponding height, and by the sine, and divided by the radius: but the fluxion of the circle multiplied by the sine and divided by the radius, is equal to the fluxion of the versed sine; therefore the ordinate multiplied by its fluxion is equal to the initial height multiplied by the fluxion of the versed sine in the corresponding circle of curvature; and *the square of the ordinate is equal to the rectangle contained by the initial height and twice the versed sine, increased by a constant quantity.* Now at the highest point of the curve, the versed sine becomes equal to the diameter, and the square of the initial height to the rectangle contained by the initial height and twice the diameter, with the constant quantity: the constant quantity is therefore equal to the rectangle contained by the initial height and its difference from twice the diameter: *this constant quantity is the square of the least ordinate*, and *the ordinate is every where a mean proportional between the greatest height and the same height diminished by twice the versed sine of the angular depression in the corresponding circle of curvature.* Again, *at the vertical point, the square of the ordinate is equal to the square of the greatest height diminished by the rectangle contained by this height and the diameter of the corresponding circle of curvature*, a rectangle which is constant for every fluid, and which may be called *the appropriate rectangle: deducting this rectangle from the square of the ordinate at the vertical point, we have the least ordinate;* which consequently vanishes *when the square of the ordinate at the vertical point is equal to the appropriate rectangle; the horizontal surface becoming in this case an asymptote to the curve, and the* *square of the greatest ordinate being equal to twice the appropriate rectangle, and the greatest ordinate to twice the diameter of the corresponding circle of curvature:* so that, *if we suppose a circle to be described, having this ordinate for a diameter, the chord of the angular elevation in this circle will be always equal to the ordinate at each point*, and *the ordinate will vary as the sine of half the angle of elevation, whenever the curve has an asymptote.* Mr. FUSS has demonstrated, in the third volume of the *Acta Petropolitana*, some properties of the arch of equilibrium under the pressure of a fluid, which is the same as one species of the curves here considered. The series given by EULER in the second part of the same volume, for the elastic curve, may also be applied to these curves.

IV. *Application to the Elevation of particular Fluids.*

The simplest phenomena, which afford us data for determining the fundamental properties of the superficial cohesion of fluids, are their elevation and depression between plates and in capillary tubes, and their adhesion to the surfaces of solids which are raised in a horizontal situation to a certain height above the general surface of the fluids. When the distance of a pair of plates, or the diameter of a tube, is very minute, the curvature may be considered as uniform, and the appropriate rectangle may readily be deduced from the elevation, recollecting that the curvature in a capillary tube is double, and the height therefore twice as great as between two plates. In the case of the elevation of a fluid in contact with a horizontal surface, the ordinate may be determined from the weight required to produce a separation; and the appropriate rectangle may be found in this manner also, the angle of contact being properly considered, in this as well as in the former case. It will appear that these experiments by no means exhibit an immediate measure of the mutual attraction of the solid and fluid, as some authors have supposed.

Sir ISAAC NEWTON asserts, in his Queries, that water ascends between two plates of glass at the distance of one hundredth of an inch, to the height of about one inch; the product of the distance and the height being about .01; but this appears to be much too little. In the best experiment of MUSSCHENBROEK, with a tube, half of the product was .0196; in several of WEITBRECHT, apparently very accurate, .0214. In MONGE's experiments on plates, the product was 2.6 or 2.7 lines, about .0210. Mr. ATWOOD says that for tubes, the product is .0530, half of which is .0265. Until more accurate experiments shall have been made, we may be contented to assume .02 for the rectangle appropriate to water, and .04 for the product of the height in a tube by its bore. Hence, when the curve becomes infinite, is greatest ordinate is .2, and the height of the vertical portion, or the height of ascent against a single vertical plane .14, or nearly one-seventh of an inch.

Now when a horizontal surface is raised from a vessel of water, the surface of the water is formed into a lintearia to which the solid is a tangent at its highest point, and if the solid be still further raised, the water will separate: the surface of the water, being horizontal at the point of contact, cannot add to the weight tending to depress the solid, which is therefore simply the hydrostatic pressure of a column of water equal in height to the elevation, in this case one-fifth of an inch, and standing on the given surface. The weight of such a column will be $50\frac{1}{2}$ grains for each square inch; and in Taylor's well known experiment the weight required was 50 grains. But when the solid employed is small, the curvature of the horizontal section of the water, which is convex externally, will tend to counteract the vertical curvature, and to diminish the height of separation; thus if a disc of an inch in diameter were employed, the curvature in this direction would perhaps be equivalent to the pressure of about one-hundredth of an inch, and might reduce the height from .2 to about .19, and the weight in the same proportion. There is however as great a diversity in the results of different experiments on the force required to elevate a solid from the surface of a fluid, as in those of the experiments in capillary tubes: and indeed the sources of error appear to be here more numerous. Mr. Achard found that a disc of glass, $1\frac{1}{2}$ inch French in diameter, required, at 69° of Fahrenheit, a weight of 91 French grains to raise it from the surface of water; this is only 37 English grains for each square inch; at $44\frac{1}{2}$° the force was $\frac{1}{14}$ greater, or $39\frac{1}{2}$ grains; the difference being $\frac{1}{343}$ for each degree of Fahrenheit. It might be inferred from these experiments, that the height of ascent in a tube of a given bore, which varies in the duplicate ratio of the height of adhesion, is diminished about $\frac{1}{180}$ for every degree of Fahrenheit that the temperature is raised above 50°; there was however probably some considerable source of error in Achard's experiments, for I find that this diminution does not exceed $\frac{1}{1000}$. The experiments of Mr. Dutour make the quantity of water raised equal to 44.1 grains for each square inch. Mr. Achard found the force of adhesion of sulfuric acid to glass, at 69° of Fahrenheit, 1.26, that of water being 1, hence the height was as .69 to 1, and its square as .47 to 1, which is the corresponding proportion for the ascent of the acid in a capillary tube, and which does not very materially differ from the proportion of .395 to 1, assigned by BARRUEL for this ascent. MUSSCHENBROEK found it .8 to 1, but his acid was probably weak. For alcohol the adhesion was as .593, the height as .715, and its square as .510: the observed proportion in a tube, according to an experiment of MUSSCHENBROEK, was about .550, according to CARRE' from .400 to .440. The experiments on sulfuric ether do not agree quite so well, but its quality is liable to very considerable variations. DUTOUR found the adhesion of alcohol .58, that of water being 1.

With respect to mercury, it has been shown by Professor CASBOIS of Metz, and by others, that its depression in tubes of glass depends on the imperfection of the contact, and that when it has been boiled in the tube often enough to expel all foreign particles, the surface may even become concave instead of convex, and the depression be converted into an elevation. But in barometers, constructed according to the usual methods, the angle of the mercury will be found to differ little from 140°; and in other experiments, when proper precautions are taken, the inclination will be nearly the same. The determination of this angle is necessary for finding the appropriate rectangle for the curvature of the surface of mercury, together with the observations of the quantity of depression in tubes of a given diameter. The table published by Mr. CAVENDISH from the experiments of his father, Lord CHARLES CAVENDISH, appears to be best suited for this purpose. I have constructed a diagram, according to the principles already laid down, for each case, and I find that the rectangle which agrees best with the phenomena is .01. The mean depression is always .015, divided by the diameter of the tube: and in tubes less than half an inch in diameter, the curve is very nearly elliptic, and the central depression in the tube of a barometer may be found by deducting from the corresponding mean depression the square root of one-thousandth part of its diameter. There is reason to suspect a slight inaccuracy towards the middle of Lord CHARLES CAVENDISH's Table, from a comparison with the calculated mean depression, as well as from the results of the mechanical construction. The ellipsis approaching nearest to the curve may be determined by the solution of a biquadratic equation.

Diameter in inches.	Grains in an inch. C.	Mean depression by calculation. Y.	Central depression by observation. C.	Central depression by formula. Y.	Central depresson by diagram Y.	Marginal depression by diagram. Y.
.6	972	.025	.005	(.001)	.005	.066
.5	675	.030	.007	.008	.007	.067
.4	432	.037	.015	.017	.012	.069
.35	331	.043	.025	.024	.017	.072
.30	243	.050	.036	.033	.027	.079
.25	169	.060	.050	.044	.038	.086
.20	108	.075	.067	.061	.056	.096
.15	61	.100	.092	.088	.085	.116
.10	27	.150	.140	.140	.140	.161

The square root of the rectangle .01, or .1, is the ordinate where the curve would become vertical if it were continued; but in order to find the height at which it adheres to a vertical surface, we must diminish this ordinate in the proportion of the sine of 25° to the sine of 45°, and it will become .06, for the actual depression in this case. The elevation of the mercury that adheres to the lower horizontal surface of a piece of glass, and the thickness at which a quantity of mercury will stand when spread out on glass, supposing the angle of contact still 140°, are found, by taking the proportion of the sines of 20° and of 70° to the sine of 45°, and are therefore .0484 and .1330 respectively. If, instead of glass, we employed any surface capable of being wetted by mercury, the height of elevation would be .141, and this is the limit of the thickness of a wide surface of mercury supported by a substance wholly incapable of attracting it. Now the hydrostatic pressure of a column of mercury .0484 in thickness on a disc of one inch diameter would be 131 grains; to this the surrounding elevation of the fluid will add about 11 grains for each inch of the circumference, with some deduction for the effect of the contrary curvature of the horizontal section, tending to diminish the height; and the apparent cohesion thus exhibited will be about 160 grains, which is a little more than four times as great as the apparent cohesion of glass and water. With a disc 11 lines in diameter Mr. DUTOUR found it 194 French grains, which is equivalent to 152 English grains, instead of 160, for an inch, a result which is sufficient to confirm the principles of the calculation. The depth of a quantity of mercury standing on glass I have found by actual observation, to agree precisely with this calculation. SEGNER says that the depth was .1358, both on glass and on paper: the difference is very trifling, but this measure is somewhat too great for glass, and too small for paper, since it appears from DUTOUR's experiments, that the attraction of paper to mercury is extremely weak.

If a disc of a substance capable of being wetted by mercury, an inch in diameter, were raised from its surface in a position perfectly horizontal, the apparent cohesion should be 381 grains, taking .141 as the height: and for a French circular inch, 433 grains, or 528 French grains. Now, in the experiments of MORVEAU, the cohesion of a circular inch of gold to the surface of mercury appeared to be 446 grains, of silver 429, of tin 418, of lead 397, of bismuth 372, of zinc 204, of copper 142, of metallic antimony 126, of iron 115, of cobalt 8: and this order is the same with that in which the metals are most easily amalgamated with mercury. It is probable that such an amalgamation actually took place in some of the experiments, and affected their results, for the process of amalgamation may often be observed to begin almost at the instant of contact of silver with mercury; and the want of perfect horizontality appears in a slight degree to have affected them all. A deviation of one-fiftieth of an inch would be sufficient to have produced the difference between 446 grains and 528; and it is not impossible that all the differences, as far down as bismuth, may have been accidental. But if we suppose the gold only to have been perfectly wetted by the mercury, and all the other numbers to be in due proportions, we may find the appropriate angle for each substance by deducting from 180°, twice the angle of which the sine is to the radius as the apparent cohesion of each to 446 grains; that is, for gold .1, for silver about .97, for tin .95, for lead .90, for bismuth .85, for zinc .46, for copper .32, for antimony .29, for iron .26, and for cobalt .02, neglecting the surrounding elevation, which has less effect in proportion as the surface employed is larger. GELLERT found the depression of melted lead in a tube of glass multiplied by the bore equal to about .0054.

It would perhaps be possible to pursue these principles so far as to determine in many cases the circumstances under which a drop of any fluid would detach itself from a given surface. But it is sufficient to infer, from the law of the superficial cohesion of fluids, that the linear dimensions of similar drops depending from a horizontal surface must vary precisely in the same ratio as the heights of ascent of the respective fluids against a vertical surface, or as the square root of the heights of ascent in a given tube: hence the magnitudes of similar drops of different fluids must vary as the cubes of the square roots of the heights of ascent in a tube. I have measured the heights of ascent of water and of diluted spirit of wine in the same tube, and I found them nearly as 100 to 64: a drop of water falling from a large sphere of glass weighed 1.8 grains, a drop of the spirit of wine about .85, instead of .82, which is nearly the weight that would be inferred from the consideration of the heights of ascent, combined with that of the specific gravities. We may form a conjecture respecting the probable magnitude of a drop by inquiring what must be the circumference of the fluid, that would support by its cohesion the weight of a hemisphere depending from it: this must be the same as that of a tube, in which the fluid would rise to the height of one-third of its diameter; and the square of the diameter must be three times as great as the appropriate product; or, for water .12; whence the diameter would be .35, or a little more than one-third of an inch, and the weight of the hemisphere would be 2.8 grains. If more water were added internally, the cohesion would be overcome, and the drop would no longer be suspended, but it is not easy to calculate what precise quantity of water would be separated with it. The form of a bubble of air rising in water is determined by the cohesion of the internal surface of the water exactly in the same manner as the form of a drop of water in the air. The delay of a bubble of air at the bottom of a vessel appears to be occasioned by a deficiency of the pressure of the water between the air and the vessel; it is nearly analogous to the experiment of making a piece of wood remain immersed in water, when perfectly in contact with the bottom of the vessel containing it. This experiment succeeds however far more readily with mercury, since the capillary cohesion of the mercury prevents its insinuating itself under the wood.

V. *Of apparent Attractions and Repulsions.*

The apparent attraction of two floating bodies, round both of which the fluid is raised by cohesive attraction, is produced by the excess of the atmospheric pressure on the remote sides of the solids above its pressure on their neighbouring sides: or, if the experiments are performed in a vacuum, by the equivalent hydrostatic pressure or suction derived from the weight and immediate cohesion of the intervening fluid. This force varies ultimately in the inverse ratio of the square of the distance; for, if two plates approach each other, the height of the fluid that rises between them is increased in the simple inverse ratio of the distance; and the mean action, or negative pressure, of the fluid on each particle of the surface is also increased in the same ratio. When the floating bodies are both surrounded by a depression, the same law prevails, and its demonstration is still more simple and obvious. The repulsion of a wet and a dry body does not appear to follow the same proportion: for it by no means approaches to infinity upon the supposition of perfect contact; its maximum is measured by half the sum of the elevation and depression on the remote sides of the substances, and as the distance increases, this maximum is only diminished by a quantity, which is initially as the square of the distance. The figures of the solids concerned modify also sometimes the law of attraction, so that, for bodies surrounded by a depression, there is sometimes a maximum, beyond which the force again diminishes: and it is hence that a light body floating on mercury, in a vessel little larger than itself, is held in a stable equilibrium without touching the sides. The reason of this will become apparent, when we examine the direction of the surface necessarily assumed by the mercury in order to preserve the appropriate angle of contact, the tension acting with less force when the surface attaches itself to the angular termination of the float in a direction less horizontal.

The apparent attraction produced between solids by the interposition of a fluid does not depend on their being partially immersed in it; on the contrary, its effects are still more powerfully exhibited in other situations; and, when the cohesion between two solids is increased and extended by the intervention of a drop of water or of oil, the superficial cohesion of these fluids is fully sufficient to explain the additional effect. When wholly immersed in water, the cohesion between two pieces of glass is little or not at all greater than when dry: but if a small portion only of a fluid be interposed, the curved surface, that it exposes to the air, will evidently be capable of resisting as great a force as it would support from the pressure of the column of fluid that it is capable of sustaining in a vertical situation; and in order to apply this force, we must employ in the separation of the plates, as great a force as is equivalent to the pressure of a column appropriate to their distance MORVEAU found that two discs of glass, 3 inches French in diameter, at the distance of one-tenth of a line, appeared to cohere with a force of 4719 grains, which is equivalent to the pressure of a column 23 lines in height: hence the product of the height and the distance of the plates is 2.3 lines, instead of 2.65, which was the result of MONGE'S experiments on the actual ascent of water. The difference is much smaller than the difference of the various experiments on the ascent of fluids; and it may easily have arisen from a want of perfect parallelism in the plates; for there is no force tending to preserve this parallelism. The error, in the extreme case of the plates coming into contact at one point, may reduce the apparent cohesion to one half.

The same theory is sufficient to explain the law of the force by which a drop is attracted towards the junction of two plates inclined to each other, and which is found to vary in the inverse ratio of the square of the distance; whence it was inferred by NEWTON that the primitive force of cohesion varies in the simple inverse ratio of the distance, while other experiments lead us to suppose that cohesive forces in general vary in the direct ratio of the distance. But the difficulty is removed by considering the state of the marginal surface of the drop. If the plates were parallel, the capillary action would be equal on both sides of the drop: but when they are inclined, the curvature of the surface at the thinnest part requires a force proportionate to the appropriate height to counteract it; and this force is greater than that which acts on the opposite side. But if the two plates are inclined to the horizon, the deficiency may be made up by the hydrostatic weight of the drop itself;

and the same inclination will serve for a larger or a smaller drop at the same place. Now when the drop approaches to the line of contact, the difference of the appropriate heights for a small drop of a given diameter will increase as the square of the distance decreases; for the fluxion of the reciprocal of any quantity varies inversely as the square of that quantity: and, in order to preserve the equilibrium, the sine of the angle of elevation of the two plates must be nearly in the inverse ratio of the square of the distance of the drop from the line of contact, as it actually appears to have been in HAUKSBEE's experiments.

VI. *Physical Foundation of the Law of superficial Cohesion.*

We have now examined the principal phenomena which are reducible to the simple theory of the action of the superficial particles of a fluid. We are next to investigate the natural foundations upon which that theory appears ultimately to rest. We may suppose the particles of liquids, and probably those of solids also, to possess that power of repulsion, which has been demonstratively shown by NEWTON to exist in aeriform fluids, and which varies in the simple inverse ratio of the distance of the particles from each other. In airs and vapours this force appears to act uncontrolled; but in liquids, it is overcome by cohesive force, while the particles still retain a power of moving freely in all directions; and in solids the same cohesion is accompanied by a stronger or weaker resistance to all lateral motion, which is perfectly independent of the cohesive force, and which must be cautiously distinguished from it. It is simplest to suppose the force of cohesion nearly or perfectly constant in its magnitude, throughout the minute distance to which it extends, and owing its apparent diversity to the contrary action of the repulsive force, which varies with the distance. Now in the internal parts of a liquid these forces hold each other in a perfect equilibrium, the particles being brought so near that the repulsion becomes precisely equal to the cohesive force that urges them together: but whenever there is a curved or angular surface, it may be found by collecting the actions of the different particles, that the cohesion must necessarily prevail over the repulsion, and must urge the superficial parts inwards with a force proportionate to the curvature, and thus produce the effect of a uniform tension of the surface. For, if we consider the effect of any two particles in a curved line on a third at an equal distance beyond them, we shall find that the result of their equal attractive forces bisects the angle formed by the lines of direction; but that the result of their repulsive forces, one of which is twice as great as the other, divides it in the ratio of one to two, forming with the former result an angle equal to one-sixth of the whole; so that the addition of a third force is necessary in order to retain these two results in equilibrium; and this force must be in a constant ratio to the evanescent angle which is the measure of the curvature, the distance of the particles being constant. The same reasoning may be applied to all the particles which are within the influence of the cohesive force: and the conclusions are equally true if the cohesion is not precisely constant, but varies less rapidly than the repulsion.

VII. *Cohesive Attraction of Solids and Fluids.*

When the attraction of the particles of a fluid for a solid is less than their attraction for each other, there will be an equilibrium of the superficial forces, if the surface of the fluid make with that of the solid a certain angle, the versed sine of which is to the diameter, as the mutual attraction of the fluid and solid particles is to the attraction of the particles of the fluid among each other. For, when the fluid is surrounded by a vacuum or by a gas, the cohesion of its superficial particles acts with full force in producing a pressure; but when it is any where in contact with a solid substance of the same attractive power with itself, the effects of this action must be as much destroyed as if it were an internal portion of the fluid. Thus, if we imagined a cube of water to have one of its halves congealed, without any other alteration of its properties, it is evident that its form and the equilibrium of the cohesive forces would remain undisturbed: the tendency of the new angular surface of the fluid water to contract would therefore be completely destroyed by the contact of a solid of equal attractive force. If the solid were of smaller attractive force, the tendency to contract would only be proportionate to the difference of the attractive forces or densities, the effect of as many of the attractive particles of the fluid being neutralised, as are equivalent to a solid of a like density or attractive power. For a similar reason, the tendency of a fluid to contract the sum of the surfaces of itself and a contiguous solid, will be simply as the density of the solid, or as the mutual attractive force of the solid and fluid. And it is indifferent whether we consider the pressure produced by these supposed superficial tensions, or the force acting in the direction of the surfaces to be compared. We may therefore inquire into the conditions of equilibrium of the three forces acting on the angular particles, one in the direction of the surface of the fluid only, a second in that of the common surface of the solid and fluid, and the third in that of the exposed surface of the solid. Now, supposing the angle of the fluid to be obtuse, the whole superficial cohesion of the fluid being represented by the radius, the part which acts in the direction of the surface of the solid will be proportional to the cosine of the inclination; and this force, added to the force of the solid, will be equal to the force of the common surface of the solid and fluid, or to the differences of their forces; consequently, the cosine added to twice the force of the solid, will be equal to the whole force of the fluid, or to the radius: hence the force of the solid is represented by half the difference between the cosine and the radius, or by half the versed sine; or, if the force of the fluid be represented by the diameter, the whole versed sine will indicate the force of the solid. And the same result follows when the angle of the fluid is acute. Hence we may infer, that if the solid have half the attractive force of the fluid, the surfaces will be perpendicular; and this seems in itself reasonable, since two rectangular edges of the solid are equally near to the angular particles with one of the fluid, and we may expect a fluid to rise and adhere to the surface of every solid more than half as attractive as itself; a conclusion which CLAIRAUT has already inferred, in a different manner, from principles which he has but cursorily investigated, in his treatise on the figure of the earth.

The versed sine varies as the square of the sine of half the angle: the force must therefore be as the square of the height to which the fluid may be elevated in contact with a horizontal surface, or nearly as the square of the number of grains expressing the apparent cohesion. Thus, according to the experiments of MORVEAU, on the suppositions already premised, we may infer that the mutual attraction of the particles of mercury being unity, that of mercury for gold will be .1 or more, that of silver about .94, of tin .90, of lead .81, of bismuth .72, of zinc .21, of copper .10, of antimony .08, of iron .07, and of cobalt .0004. The attraction of glass for mercury will be about one-sixth of the mutual attraction of the particles of mercury: but when the contact is perfect, it appears to be considerably greater.

Although the whole of this reasoning on the attraction of solids is to be considered rather as an approximation than as a strict demonstration, yet we are amply justified in concluding, that all the phenomena of capillary action may be accurately explained and mathematically demonstrated from the general law of the equable tension of the surface of a fluid, together with the consideration of the angle of contact appropriate to every combination of a fluid with a solid. Some anomalies, noticed by MUSSCHENBROEK and others, respecting in particular the effects of tubes of considerable lengths, have not been considered: but there is great reason to suppose that either the want of uniformity in the bore, or some similar inaccuracy, has been the cause of these irregularities, which have by no means been sufficiently confirmed to afford an objection to any theory. The principle, which has been laid down respecting the contractile powers of the common surface of a solid and a fluid, is confirmed by an observation which I have made on the small drops of oil which form themselves on water. There is no doubt but that this cohesion is in some measure independent of the chemical affinities of the substances concerned: tallow when solid has a very evident attraction for the water out of which it is raised; and the same attraction must operate upon an unctuous fluid to cause it to spread on water, the fluidity of the water allowing this powerful agent to exert itself with an unresisted velocity. An oil which has thus been spread is afterwards collected, by some irregularity of attraction, into thin drops, which the slightest agitation again dissipates: their surface forms a very regular curve, which terminates abruptly in a surface perfectly horizontal: now it follows from the laws of hydrostatics, that the lower surface of these drops must constitute a curve, of which the extreme inclination to the horizon is to the inclination of the upper surface as the specific gravity of the oil to the difference between its specific gravity and that of water: consequently since the contractile forces are held in equilibrium by a force which is perfectly horizontal, their magnitude must be in the ratio that has been already assigned; and it may be assumed as consonant both to theory and to observation, that the contractile force of the common surface of two substances, is proportional, other things being equal, to the difference of their densities. Hence, in order to explain the experiments of BOYLE on the effects of a combination of fluids in capillary tubes, or any other experiments of a similar nature, we have only to apply the law of an equable tension, of which the magnitude is determined by the difference of the attractive powers of the fluids.

I shall reserve some further illustrations of this subject for a work which I have long been preparing for the press, and which I flatter myself will contain a clear and simple explanation of the most important parts of natural philosophy. I have only thought it right, in the present Paper, to lay before the Royal Society, in the shortest possible compass, the particulars of an original investigation, tending to explain some facts and establish some analogies, which have hitherto been obscure and unintelligible.

IV. *Concerning the State in which the true Sap of Trees is deposited during Winter. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P.R.S.*

Read January 24, 1805.

MY DEAR SIR,

It is well known that the fluid, generally called the Sap in trees, ascends in the spring and summer from their roots, and that in the autumn and winter it is not, in any considerable quantity, found in them; and I have observed in a former Paper, that this fluid rises wholly through the alburnum, or sap-wood. But Du Hamel and subsequent naturalists have proved, that trees contain another kind of sap, which they have called the true, or peculiar juice, or sap of the plant. Whence this fluid originates does not appear to have been agreed by naturalists; but I have offered some facts to prove that it is generated by the leaf;* and that it differs from the common aqueous sap owing to changes it has undergone in its circulation through that organ: and I have contended that from this fluid (which Du Hamel has called the *suc propre*, and which I will call the true sap,) the whole substance, which is annually added to the tree, is derived. I shall endeavour in the present Paper to prove that this fluid, in an inspissated state, or some concrete matter deposited by it, exists during

* See Phil. Trans. of 1801, page 336.

the winter in the alburnum, and that from this fluid, or substance, dissolved in the ascending aqueous sap, is derived the matter which enters into the composition of the new leaves in the spring, and thus furnishes those organs, which were not wanted during the winter, but which are essential to the further progress of vegetation.

Few persons at all conversant with timber are ignorant, that the alburnum, or sap-wood of trees, which are felled in the autumn or winter, is much superior in quality to that of other trees of the same species, which are suffered to stand till the spring, or summer: it is at once more firm and tenacious in its texture, and more durable. This superiority in winter-felled wood has been generally attributed to the absence of the sap at that season; but the appearance and qualities of the wood seem more justly to warrant the conclusion, that some substance has been added to, instead of taken from it, and many circumstances induced me to suspect that this substance is generated, and deposited within it, in the preceding summer and autumn.

DU HAMEL has remarked, and is evidently puzzled with the circumstance, that trees perspire more in the month of August, when the leaves are full grown, and when the annual shoots have ceased to elongate, than at any earlier period; and we cannot suppose the powers of vegetation to be thus actively employed, but in the execution of some very important operaration. Bulbous and tuberous roots are almost wholly generated after the leaves and stems of the plants, to which they belong, have attained their full growth; and I have constantly found, in my practice as a farmer, that the produce of my meadows has been immensely increased when the herbage of the preceding year had remained to perform its proper office till the end of the autumn, on ground which had been mowed early in the summer. Whence I have been led to imagine, that the leaves, both of trees and herbaceous plants, are alike employed, during the latter part of the summer, in the preparation of matter calculated to afford food to the expanding buds and blossoms of the succeeding spring, and to enter into the composition of new organs of assimilation.

If the preceding hypothesis be well founded, we may expect to find that some change will gradually take place in the qualities of the aqueous sap of trees during its ascent in the spring; and that any given portion of winter-felled wood will at the same time possess a greater degree of specific gravity, and yield a larger quantity of extractive matter, than the same quantity of wood which has been felled in the spring or in the early part of the summer. To ascertain these points I made the experiments, an account of which I have now the honour to lay before you.

As early in the last spring as the sap had risen in the sycamore and birch, I made incisions into the trunks of those trees, some close to the ground, and others at the elevation of seven feet, and I readily obtained from each incision as much sap as I wanted. Ascertaining the specific gravity of the sap of each tree, obtained at the different elevations, I found that of the sap of the sycamore with very little variation, in different trees, to be 1.004 when extracted close to the ground, and 1.008 at the height of seven feet. The sap of the birch was somewhat lighter; but the increase of its specific gravity, at greater elevation, was comparatively the same. When extracted near the ground the sap of both kinds was almost free from taste; but when obtained at a greater height, it was sensibly sweet. The shortness of the trunks of the sycamore trees, which were the subjects of my experiments, did not permit me to extract the sap at a greater elevation than seven feet, except in one instance, and in that, at twelve feet from the ground, I obtained a very sweet fluid, whose specific gravity was 1.012.

I conceived it probable, that if the sap in the preceding cases derived any considerable portion of its increased specific gravity from matter previously existing in the alburnum, I should find some diminution of its weight, when it had continued to flow some days from the same incision, because the alburnum in the vicinity of that incision would, under such circumstances, have become in some degree exhausted: and on comparing the specific gravity of the sap which had flowed from a recent and an old incision, I found that from the old to be reduced to 1.002, and that from the recent one to remain 1.004, as in the preceding cases, the incision being made close to the ground. Wherever extracted, whether close to the ground, or at some distance from it, the sap always appeared to contain a large portion of air.

In the experiments to discover the variation in the specific gravity of the alburnum of trees at different seasons, some obstacles to the attainment of any very accurate results presented themselves. The wood of different trees of the same species, and growing in the same soil, or that taken from different parts of the same tree, possesses different degrees of solidity; and the weight of every part of the alburnum appears to increase with its age, the external layers being the lightest. The solidity of wood varies also with the greater or less rapidity of its growth. These sources of error might apparently have been avoided by cutting off, at different seasons, portions of the same trunk or branch: but the wound thus made might, in some degree, have impeded the due progress of the sap in its ascent, and the part below might have been made heavier by the stagnation of the sap, and that above lighter by privation of its proper quantity of nutriment. The most eligible method therefore, which occurred to me, was to select and mark in the winter some of the poles of an oak coppice, where all are of equal age, and where many, of the same size and growing with equal vigour, spring from the same stool. One half of the poles which I marked and numbered were cut on the 31st of December, 1803, and the remainder on the 15th of the following May, when the leaves were nearly half grown. Proper marks were put to distinguish the winter-felled from the summer-felled poles, the bark being left on all, and all being placed in the same situation to dry.

In the beginning of August I cut off nearly equal portions from a winter and summer-felled pole, which had both grown on the same stool; and both portions were then put in a situation, where, during the seven succeeding weeks, they were kept very warm by a fire. The summer-felled wood was, when put to dry, the most heavy; but it evidently contained much more water than the other, and, partly at least, from this cause, it contracted much more in drying. In the beginning of October both kinds appeared to be perfectly dry, and I then ascertained the specific gravity of the winter-felled wood to be 0.679, and that of the summer-felled wood to be 0.609; after each had been immersed five minutes in water.

This difference of ten *per cent.* was considerably more than

I had anticipated, and it was not till I had suspended and taken off from the balance each portion, at least ten times, that I ceased to believe that some error had occurred in the experiment: and indeed I was not at last satisfied till I had ascertained by means of compasses adapted to the measurement of solids, that the winter-felled pieces of wood were much less than the others which they equalled in weight.

The pieces of wood, which had been the subjects of these experiments, were again put to dry, with other pieces of the same poles, and I yesterday ascertained the specific gravity of both with scarcely any variation in the result. But when I omitted the medulla, and parts adjacent to it, and used the layers of wood which had been more recently formed, I found the specific gravity of the winter-felled wood to be only 0.583, and that of the summer-felled to be 0.533; and trying the same experiment with similar pieces of wood, but taken from poles which had grown on a different stool, the specific gravity of the winter-felled wood was 0.588, and that of the summer-felled 0.534.

It is evident that the whole of the preceding difference in the specific gravity of the winter and summer-felled wood might have arisen from a greater degree of contraction in the former kind, whilst drying; I therefore proceeded to ascertain whether any given portion of it, by weight, would afford a greater quantity of extractive matter, when steeped in water. Having therefore reduced to small fragments 1000 grains of each kind, I poured on each portion six ounces of boiling water; and at the end of twenty-four hours, when the temperature of the water had sunk to 60°, I found that the winter-felled wood had communicated a much deeper colour to the water in which it had been infused, and had raised its specific gravity to 1.002. The specific gravity of the water in which the summer-felled wood had, in the same manner, been infused was 1.001. The wood in all the preceding cases was taken from the upper parts of the poles, about eight feet from the ground.

Having observed, in the preceding experiments, that the sap of the sycamore became specifically lighter when it had continued to flow during several days from the same incision, I concluded that the alburnum in the vicinity of such incision had been deprived of a larger portion of its concrete or inspissated sap than in other parts of the same tree: and I therefore suspected that I should find similar effects to have been produced by the young annual shoots and leaves; and that any given weight of the alburnum in their vicinity would be found to contain less extractive matter than an equal portion taken from the lower parts of the same pole, where no annual shoots or leaves had been produced.

No information could in this case be derived from the difference in the specific gravity of the wood; because the substance of every tree is most dense and solid in the lower parts of its trunk: and I could on this account judge only from the quantity of extractive matter which equal portions of the two kinds of wood would afford. Having therefore reduced to pieces several equal portions of wood taken from different parts of the same poles, which had been felled in May, I poured on each portion an equal quantity of boiling water, which I suffered to remain twenty hours, as in the preceding experiments: and I then found that in some instances the wood from the lower, and in others that from the upper parts of the poles, had given to the water the deepest colour and greatest degree of specific gravity; but that all had afforded much extractive matter, though in every instance the quantity yielded was much less than I had, in all cases, found in similar infusions of winter-felled wood.

It appears, therefore, that the reservoir of matter deposited in the alburnum is not wholly exhausted in the succeeding spring: and hence we are able to account for the several successions of leaves and buds which trees are capable of producing when those previously protruded have been destroyed by insects, or other causes; and for the extremely luxuriant shoots, which often spring from the trunks of trees, whose branches have been long in a state of decay.

I have also some reasons to believe that the matter deposited in the alburnum remains unemployed in some cases during several successive years: it does not appear probable that it can be all employed by trees which, after having been transplanted, produce very few leaves, or by those which produce neither blossoms nor fruit. In making experiments in 1802, to ascertain the manner in which the buds of trees are reproduced, I cut off in the winter all the branches of a very large old pear-tree, at a small distance from the trunk; and I pared off, at the same time, the whole of the lifeless external bark. The age of this tree, I have good reasons to believe, somewhat exceeded two centuries: its extremities were generally dead; and it afforded few leaves, and no fruit; and I had long expected every successive year to terminate its existence. After being deprived of its external bark, and of all its buds, no marks of vegetation appeared in the succeeding spring, or early part of the summer: but in the beginning of July numerous buds penetrated through the bark in every part, many leaves of large size every where appeared, and in the autumn every part was covered with very vigorous shoots exceeding, in the aggregate, two feet in length. The number of leaves which, in this case, sprang at once from the trunk and branches appeared to me greatly to exceed the whole of those, which the tree had born in the three preceding seasons; and I cannot believe that the matter which composed these buds and leaves could have been wholly prepared by the feeble vegetation and scanty foliage of the preceding year.

But whether the substance which is found in the alburnum of winter-felled trees, and which disappears in part in the spring and early part of the summer, be generated in one or in several preceding years, there seem to be strong grounds of probability, that this substance enters into the composition of the leaf: for we have abundant reason to believe that this organ is the principal agent of assimilation; and scarcely any thing can be more contrary to every conclusion we should draw from analogical reasoning and comparison of the vegetable with the animal economy, or in itself more improbable, than that the leaf, or any other organ, should singly prepare and assimilate immediately from the crude aqueous sap, that matter which composes itself.

It has been contended* that the buds themselves contain the nutriment necessary for the minute unfolding leaves: but trees possess a power to reproduce their buds, and the matter necessary to form these buds must evidently be derived from some other source: nor does it appear probable that the young leaves very soon enter on this office: for the experiments of

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* THOMSON's Chemistry.

INGENHOUZ prove that their action on the air which surrounds them is very essentially different from that of full grown leaves. It is true that buds in many instances will vegetate, and produce trees, when a very small portion only of alburnum remains attached to them; but the first efforts of vegetation in such buds are much more feeble than in others to which a larger quantity of alburnum is attached, and therefore we have, in this case, no grounds to suppose that the leaves derive their first nutriment from the crude sap.

It is also generally admitted, from the experiments of BONNET and DU HAMEL, which I have repeated with the same result, that in the cotyledons of the seed is deposited a quantity of nutriment for the bud, which every seed contains; and though no vessels can be traced* which lead immediately from the cotyledons to the bud or plumula, it is not difficult to point out a more circuitous passage, which is perfectly similar to that through which I conceive the sap to be carried from the leaves to the buds, in the subsequent growth of the tree; and I am in possession of many facts to prove that seedling trees, in the first stage of their existence, depend entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and in many instances killed, by being put to vegetate in rich mould.

We have much more decisive evidence that bulbous and tuberous rooted plants contain the matter within themselves which subsequently composes their leaves; for we see them vegetate even in dry rooms, on the approach of spring; and many bulbous rooted plants produce their leaves and flowers with nearly the same vigour by the application of water only, as they do when growing in the best mould. But the water in this case, provided that it be perfectly pure, probably affords little or no food to the plant, and acts only by dissolving the matter prepared and deposited in the preceding year; and hence the root becomes exhausted and spoiled: and Hassenfratz found that the leaves and flowers and roots of such plants afforded no more carbon than he had proved to exist in bulbous roots of the same weight, whose leaves and flowers had never expanded.

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* HEDWIG.

As the leaves and flowers of the hyacinth, in the preceding case, derived their matter from the bulb, it appears extremely probable that the blossoms of trees receive their nutriment from the alburnum, particularly as the blossoms of many species precede their leaves: and, as the roots of plants become weakened and apparently exhausted, when they have afforded nutriment to a crop of seed, we may suspect that a tree, which has borne much fruit in one season, becomes in a similar way exhausted, and incapable of affording proper nutriment to a crop in the succeeding year. And I am much inclined to believe that were the wood of a tree in this state accurately weighed, it would be found specifically lighter than that of a similar tree, which had not afforded nutriment to fruit or blossoms, in the preceding year, or years.

If it be admitted that the substance which enters into the composition of the first leaves in the spring is derived from matter which has undergone some previous preparation within the plant, (and I am at a loss to conceive on what grounds this can be denied, in bulbous and tuberous rooted plants at least,) it must also be admitted that the leaves which are generated in the summer derive their substance from a similar source; and this cannot be conceded without a direct admission of the existence of vegetable circulation, which is denied by so many eminent naturalists. I have not, however, found in their writings a single fact to disprove its existence, nor any great weight in their arguments, except those drawn from two important errors in the admirable works of HALES and DU HAMEL, which I have noticed in a former memoir. I shall therefore proceed to point out the channels, through which I conceive the circulating fluids to pass.

When a seed is deposited in the ground, or otherwise exposed to a proper degree of heat and moisture, and exposure to air, water is absorbed by the cotyledons and the young radicle or root is emitted. At this period, and in every subsequent stage of the growth of the root, it increases in length by the addition of new parts to its apex, or point, and not by any general distension of its vessels and fibres; and the experiments of BONNET and DU HAMEL leave little grounds of doubt, but that the new matter which is added to the point of the root descends from the cotyledons. The first motion therefore of the fluids in plants is downwards, towards the point of the root; and the vessels which appear to carry them, are of the same kind with those which are subsequently found in the bark, where I have, on a former occasion, endeavoured to prove that they execute the same office.

In the last spring I examined almost every day the progressive changes which take place in the radicle emitted by the horse chesnut: I found it, at its first existence, and until it was some weeks old, to be incapable of absorbing coloured infusions, when its point was taken off, and I was totally unable to discover any alburnous tubes, through which the sap absorbed from the ground, in the subsequent growth of the tree, ascends: but when the roots were considerably elongated, alburnous tubes formed; and as soon as they had acquired some degree of firmness in their consistence, they appeared to enter on their office of carrying up the aqueous sap, and the leaves of the plumula then, and not sooner, expanded.

The leaf contains at least three kinds of tubes: the first is what, in a former Paper, I have called the central vessel, through which the aqueous sap appears to be carried, and through which coloured infusions readily pass, from the alburnous tubes into the leaf-stalk. These vessels are always accompanied by spiral tubes, which do not appear to carry any liquid: but there is another vessel which appears to take its origin from the leaf, and which descends down the internal bark, and contains the true or prepared sap. When the leaf has attained its proper growth, it seems to perform precisely the office of the cotyledon; but being exposed to the air, and without the same means to acquire, or the substance to retain moisture, it is fed by the alburnous tubes and central vessels. The true sap now appears to be discharged from the leaf, as it was previously from the cotyledon, into the vessels of the bark, and to be employed in the formation of new alburnous tubes between the base of the leaf and the root. From these alburnous tubes spring other central vessels and spiral tubes, which enter into and possibly give existence to, other leaves; and thus by a repetition of the same process the young tree or annual shoot continues to acquire new parts, which apparently are formed from the ascending aqueous sap.

But it has been proved by DU HAMEL that a fluid, similar to that which is found in the true sap vessels of the bark, exists also in the alburnum, and this fluid is extremely obvious in the fig, and other trees, whose true sap is white, or coloured. The vessels, which contain this fluid in the alburnum, are in contact with those which carry up the aqueous sap; and it does not appear probable that, in a body so porous as wood, fluids so near each other should remain wholly unmixed. I must therefore conclude that when the true sap has been delivered from the cotyledon or leaf into the returning, or true sap vessels of the bark, one portion of it secretes through the external cellular, or more probably glandular substance of the bark, and generates a new epidermis, where that is to be formed; and that the other portion of it secretes through the internal glandular substance of the bark, where one part of it produces the new layer of wood, and the remainder enters the pores of the wood already formed, and subsequently mingles with the ascending aqueous sap; which thus becomes capable of affording the matter necessary to form new buds and leaves.

It has been proved in the preceding experiments on the ascending sap of the sycamore and birch, that that fluid does not approach the buds and unfolding leaves in the spring, in the state in which it is absorbed from the earth: and therefore we may conclude that the fluid, which enters into, and circulates through the leaves of plants, as the blood through the lungs of animals, consists of a mixture of the true sap or blood of the plant with matter more recently absorbed, and less perfectly assimilated.

It appears probable that the true sap undergoes a considerable change on its mixture with the ascending aqueous sap; for this fluid in the sycamore has been proved to become more sensibly sweet in its progress from the roots in the spring, and the liquid which flows from the wounded bark of the same tree is also sweet; but I have never been able to detect the slightest degree of sweetness in decoctions of the sycamore wood in winter. I am therefore inclined to believe that the saccharine matter existing in the ascending sap is not immediately, or wholly, derived from the fluid which had circulated through the leaf in the preceding year; but that it is generated by a process similar to that of the germination of seeds, and that the same process is always going forward during the spring and summer, as long as the tree continues to generate new organs. But towards the conclusion of the summer I conceive that the true sap simply accumulates in the alburnum, and thus adds to the specific gravity of winter-felled wood, and increases the quantity of its extractive matter.

I have some reasons to believe that the true sap descends through the alburnum as well as through the bark, and I have been informed that if the bark be taken from the trunks of trees in the spring, and such trees be suffered to grow till the following winter, the alburnum acquires a great degree of hardness and durability. If subsequent experiments prove that the true sap descends through the alburnum, it will be easy to point out the cause why trees continue to vegetate after all communication between the leaves and roots, through the bark, has been intercepted: and why some portion of alburnous matter is in all trees* generated below incisions through the bark.

It was my intention this year to have troubled you with some observations on the reproduction of the buds and roots of trees;

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* I have in a former paper stated that the perpendicular shoots of the vine form an exception. I spoke on the authority of numerous experiments; but they had been made late in the summer; and on repeating the same experiments at an earlier period, I found the result in conformity with my experiments on other trees.

but as the subject of the Paper, which I have now the honour to address to you, appeared to be of more importance, I have deferred those observations to a future opportunity; and I shall at present only observe, that I conceive myself to be in possession of facts to prove that both buds and roots originate from the alburnous substance of plants, and not, as is, I believe, generally supposed, from the bark.

I am, &c.

Elton, Dec. 4,
1804.

T. ANDREW KNIGHT.

V. *On the Action of Platina and Mercury upon each other. By* Richard Chenevix, *Esq. F. R. S. M. R. I. A.* &c.

Read January 10, 1805.

Freyberg, June 3d, 1804.

On the 12th of May, 1803, I had the honour of presenting a Paper to the Royal Society, the object of which was to discover the nature of palladium, a substance just then announced to the public as a new simple metal. The experiments which I had made for this purpose led me to conclude that palladium was not what it had been stated to be, but that it was a compound of platina and mercury.

It was natural to suppose that a subject so likely to spread its influence throughout the whole domain of chemistry, and which tended even to the subversion of some of its elements, would awaken the attention of philosophers. We find accordingly, that it has become a subject of enquiry in England, France, and Germany; but the experiments which I had recommended as the least likely to fail, have been found insufficient to insure the principal result; and I have had the mortification to learn that they have been generally unsuccessful. I have even reason to believe that the nature of palladium is still considered by chemists, at least with a very few exceptions, as unascertained; and that the fixation of mercury by platina is by many regarded as visionary.

The first doubts were manifested in England; and Dr.

WOLLASTON very early denied the accuracy of my inquiries. But as he has not published his experiments, I have had no opportunity of discussing them. His opinion, however, must have such weight in the learned world, that I should have neglected a material fact in the history of palladium, if I had not mentioned it in this place.

In France the compound nature of palladium has been more generally credited. When the National Institute was informed of my experiments, a report was ordered to be made upon them, and M. GUYTON was the person appointed for the purpose. He repeated some of the experiments, and produced some of his results. His general conclusion was the same as mine.

Messrs. VAUQUELIN and FOURCROY then undertook the subject, and they were led by it to the confirmation of the recent discovery of Mons. DESCOTILS. The existence of a new metal, which that chemist had found in crude platina, received great sanction from their experiments; and thus the discussion upon palladium has established a fact which will be considered as interesting, but which would be much more so, were we not already overburthened with substances which our present ignorance obliges us to acknowledge as simple.

No sooner were these celebrated chemists convinced of the existence of a new metal in platina, than they concluded that it must play a principal part in the composition of palladium. Shortly after this, in a note to a letter from M. PROUST to M. VAUQUELIN, in which M. PROUST expresses his astonishment concerning all he has read upon palladium, Mess. FOURCROY and VAUQUELIN further declare, as their opinion, that this compound metal does not contain mercury, but is formed of platina and the new metal. Whether this new substance does or does not play a principal part in the formation of palladium, could not be ascertained at the time my experiments were made, because the new metal itself was not then known. But from all that Mess. FOURCROY and VAUQUELIN have stated, in such of their different memoirs upon this subject as I have seen, the grounds of their supposition have not appeared. May we not refer their opinion, then, to that common propensity of the mind, against which M. FOURCROY has himself warned us with equal justness and eloquence on another occasion, namely, a proneness to be allured by novelty beyond the bounds of rational belief, and to convert principles which are new into principles of universal influence.

Mess. ROSE and GEHLEN * were the first among the German chemists who instituted experiments upon palladium; and M. RICHTER has also published a paper on the same subject.

The first attempt of Mess. ROSE and GEHLEN to form palladium was by the precipitation of a mixed solution of platina and mercury by green sulphate of iron. Their result was precisely that which I had observed when my operations failed altogether, and which of course was the most frequent. This method was repeated twice. The second time the precipitate of platina and mercury was boiled with muriatic acid, in order to free it from iron; but the latter trial was not more successful than the former.

Their third experiment was, what they have called, a repetition of that in which I had obtained palladium by passing a

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* *Neues Algemeines Journal der Chemie herausgegeben von Hermstadt,* KLAPROTH, RICHTER, SCHERER, TROMSDORF, und GEHLEN. *Ersten bandes funftes heft.*

current of sulphuretted hydrogen gas through a mixed solution of platina and mercury. Their method was the following. They dissolved one hundred and fifty grains of platina with four hundred and fifty of mercury, and added a solution of hydrosulphuret of potash. They obtained a precipitate which, at first, was black, afterwards gray; but the whole became black by being stirred. To be certain that all the metal was precipitated, they added an excess of sulphuret of potash, and perceived that a part of the precipitate was redissolved. The liquor was then filtered, and to that part of it, which contained the redissolved precipitate, an acid was added. From this process they obtained a yellow precipitate weighing ninety-one grains; and fifty grains of this, exposed to a strong heat, left three-eighths of a grain of platina. They obtained no palladium from that part of the precipitate which had not been redissolved; and the result of the experiment was complete failure.

I shall not make any observation upon the issue of this process, since, in this case, the best conducted is but too liable to be unsuccessful, and that without any apparent fault in the operator. But as it has been given as a repetition of one of mine, it may not be fruitless to examine how far the repetition was exact.

I had passed a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury, by which means they were precipitated together. My object was so intimately to combine sulphur with these metals, that when exposed to heat, they might (if I may be allowed the expression) be in chemical contact with it at the moment of their nascent metallic state; and as a low temperature suffices, as well to reduce those metals, as to combine palladium with sulphur, I hoped that those effects might be produced before the total dissipation of the mercury. How far my expectation was fulfilled has been stated in my former Paper.

The sulphuretted hydrogen gas which Mess. ROSE and GEHLEN presented to those metals was combined with potash. Now, in the course of docimastic lectures annually delivered by M. VAUQUELIN at the Ecole des Mines in Paris, when he was Professor at that establishment, it was his constant custom to exhibit an experiment to prove that mercury, precipitated from its solution by many of the alkaline and earthy hydrosulphurets, was redissolved by adding an excess of them.

It is moreover well known, that there is a strong affinity between potash and the oxide of platina, and also that when those substances are brought together in solution, a triple salt, but little soluble, is the result. It was to avoid these difficulties that I had employed uncombined sulphuretted hydrogen gas; for the method adopted by Mess. ROSE and GEHLEN appearing to me to be the application of two divellent forces, I presumed that it would produce a separation. The result of their experiment, which, it appears from their paper, they had not anticipated, shews the necessity of the precaution I had used. The operation which they performed to unite platina and mercury was, in fact, nearly the reverse of that which they supposed they had repeated from me, and might have been applied perhaps with a better prospect of success towards the decomposition of palladium.

Mess. ROSE and GEHLEN seem, in many parts of their paper, to question my having fused platina; and inform us that although they had exposed this metal in the furnace of the

Royal Porcelain Manufactory of Berlin, in which WEDGEWOOD's pyrometer ceased to mark the degree of heat, they could not accomplish its fusion. Many of my friends in England have however seen the buttons which I obtained, and which were not few in number. The flux which I had used was borax. But no mention is made in any one of the operations of Mess. ROSE and GEHLEN of borax having been employed.

In many of their attempts they obtained an irregular and porous mass, which of course was of a specific gravity much inferior to that of platina; and it might be inferred from their paper that the diminution of specific gravity, which I had observed, was owing to the same cause. It is true, not only that I had very often obtained such a mass, but that I had frequently also observed no diminution whatsoever in the specific gravity of the button which resulted from my operations. But all those upon which I had founded the conclusions alluded to by Mess. ROSE and GEHLEN were performed in the following manner, and have been repeated since. A Hessian crucible was filled with lamp-black, and the contents pressed hard together. The lamp-black was then hollowed out to the shape of the crucible as far as one-third from the bottom, leaving that much filled with the compressed materials; this lining, which adhered strongly to the sides of the crucible, was made extremely thin in order not to obstruct the passage of caloric. A cylindrical piece of wood, as a pencil, was then forced into the centre of the thick mass of lamp-black at the bottom, and the diameter of this rod was determined by the quantity of metal to be fused, or varied according to other circumstances at pleasure. In general the axis of the cylindrical hole was about three or four times the diameter of the basis. After withdrawing the rod the crucible was about half filled with borax. Upon this was placed the metal to be fused; and if it had been before melted into a cylindrical form, the axis of the metallic cylinder was placed horizontally, and was of course perpendicular to the axis of the cylindrical excavation at the bottom of the crucible. More borax was then added to cover the piece of metal, and another quantity of lamp-black was pressed hard over the whole in order to keep it tight together. An earthen cover was finally luted to the crucible, and in this state it was exposed to heat in a forge, in which upon another occasion, I had, in the presence of Mess. HATCHETT, HOWARD, DAVY, and others, completely melted a Hessian crucible lined and prepared in the same manner. The fuel which I used was the patent coak of Mess. DAVEY and SAWYER. In the present experiments I moderated the heat so as not materially to injure the crucible, and upon taking it out of the fire, the lining was generally found so compact and so firm that it remained in a solid mass after the crucible was broken. When the metallic cylinder occupied the space at the bottom, it was natural to suppose that it had been fused; because in no other state but that of liquidity could it have run into the mould. In order however to prevent all objections I had the precaution to make the hole of a different diameter from the metallic cylinder, and to observe whether the necessary change in the shape of the latter ensued. If, after such a test, repeated as often as required, I perceived that the metal did not vary in its specific gravity, I thought myself authorised to conclude that it was exempt from air.

M. RICHTER says that he had hoped to have put himself in possession of a considerable piece of palladium by repeating with minute accuracy the process which I had recommended as the best. He precipitated a mixed solution of platina and mercury by a solution of green sulphate of iron; and after varying the subsequent operations, to which he submitted the product he had obtained by this method, he was led to the following important conclusions amongst others of less consequence. 1st, That two metals, the separate solutions of which are not acted upon by a third body, may be acted upon, and even reduced to the metallic state, by that same body when presented to them in one and the same solution.

2dly. That mercury is capable of entering into combination with platina so, that it cannot afterwards be separated by fire. From the first of these conclusions it is evident, that metals in their metallic state are not incapable of chemical action upon each other; and from the second, that mercury can be fixed (it is purposely that I use the alchemical expression) by platina.

In addition to the chemists abovementioned, I must name two more who in Germany have been occupied by palladium. M. Tromsdorff, in a letter to the authors of the journal already quoted, mentions his having made some fruitless attempts to form this combination; and M. Klaproth, in a letter to M. Vauquelin published in the *Annales de Chimie*, for Ventose, an 12, likewise says that he could not succeed in producing palladium.

Mess. Rose and Gehlen, as well as M. Richter, had conceived from my Paper a reliance on the success of their experiments, which no words of mine had authorised, and have accused me of enforcing the truth of my results with a degree of certainty which their observations do not countenance.

M. Richter supposed that the formation of palladium was attended with no difficulty; and in general they have laid so much stress upon this charge, that I should be inclined to think my Paper had not been read by these chemists. In referring to it again, I find there is hardly a page in which I do not mention some failure, and no experiment, of the very few which occasionally succeeded, is related without my stating at the same time that it was repeatedly unsuccessful. As far as regards palladium, it is rather a narration of fruitless attempts than a description of an infallible process, and more likely to create aversion to the pursuit than to inspire a confidence of success. The course of experiments which I had made, as well before as after reading my Paper to the Society, took me up more than two months, and employed me from twelve to sixteen hours almost every day. I had frequently seven or eight operations in the forge to perform daily, and I do not exaggerate the number of attempts I made during this time, as well in the dry as in the humid way, in stating them to have been one thousand. Amongst these I had four successful operations. I persevered, because even in my failures I saw sufficient to convince me that I should quit the road to truth if I desisted. After all my labour and fatigue I cannot say that I had come nearer to my object, of obtaining more certainty in my processes. Their success was still a hazard on the dice, against which there were many chances; but till others had thrown as often as I had done, they had no solid right to deny the existence of such a combination. On this foundation none, I believe, have established such a right. Mess. Rose and Gehlen do not say how often their experiments were repeated; but it is probable that if they had been performed very often, these authors would not have neglected to mention it. M. RICHTER states his merely as preparatory to more extensive researches; and M. TROMSDORFF, as well as M. KLAPROTH, mention little more than the fact. If the German chemists have concluded against my results, they have done so without just grounds, and without having bestowed upon them that labour and assiduity for which they are usually so remarkable.

In this state of uncertainty the compound nature of palladium received an indirect, but a very able, support from some experiments of M. RITTER, the celebrated GALVANIST of Jena. M. RITTER had ascertained the rank which a great number of substances hold in a GALVANIC series, arranged according to the property they possess of becoming positive or negative when in contact with each other. He had established the following order, the preceding substance being in a *minus* relation to that which comes next. Zinc, lead, tin, iron, bismuth, cobalt, antimony, platina, gold, mercury, silver, coal, galena, crystallized tin ore, kupfer nickel, sulphur pyrites, copper pyrites, arsenical pyrites, graphite, crystallized oxide of manganese. He had the goodness to try palladium in my presence, and found it to be removed, not only from what I believed to be its constituent parts, but altogether from among the metals, and to stand between arsenical pyrites and graphite. This result led M. RITTER into a new and gêneral train of reasoning, and induced him to undertake the examination of a great number of alloys, and of a variety of amalgams. He considered the subject as a philosopher; and his operations were those of a consummate experimentalist. It would be doing him an injustice to attempt an extract of his ingenious paper, which contains a series of the most interesting experiments. I shall merely observe for the present purpose, that it very rarely happened that the mixture of two metals bore any determinate relation to the same metals when separate; that in every case the smallest variation in the proportions produced the most marked effects; and that M. RITTER has furnished us with an instrument calculated to detect the presence of such small quantities as have hitherto been considered as out of the reach of chemistry. As palladium presents a very striking instance of the anomaly, to which all compounds seem to be more or less subject, by being removed altogether from the series of simple metals, this may serve to support the other proofs of its compound nature.

One of the principal objections of those who dispute the truth of my conclusions with respect to palladium, is grounded upon the repeated failure of all the methods I had made use of in forming it; but this cannot be of very great weight, when we consider the uncertainty of many other operations of chemistry. The most simple are sometimes liable to fail: and the easiest analyses have often given different products in the hands of different chemists, who yet enjoy indisputable and equal rights to the title of accuracy. The progress which we have made in some parts of the science has not removed the obstacles which impede our advancement in others. We have no method of proving the truth of an experiment except by repeating it: yet this often tends to show nothing more than contradictory results, and consequently the fallibility of the art.

But a recent case has occurred which is perfectly analogous to that of palladium. A few years ago Professor LAMPADIUS, in distilling some substances which contained sulphur and charcoal, obtained a liquid product of a peculiar nature. He repeated his experiments, but in vain: and after many fruitless attempts abandoned his researches, and confined himself to stating the fact to the chemical world. Little notice was taken of it, and not much interest was excited by an experiment so likely to fail. Some time after this Mess. CLEMENT and DESORMES obtained the same result, and attempted to produce the substance a second time. They performed a vast number of experiments; but their success bore no proportion to their diligence and zeal. They published an account of their process and its consequences, but gained little credit, as no person was fortunate enough to produce the same substance. Many disbelieved the experiments altogether, and denied the existence of such a combination; whilst others, less inclined to doubt, attributed its formation to fortuitous circumstances which might never again occur together. In February, 1804, Professor LAMPADIUS, in distilling some pyritized wood, though with a different intent, obtained the same substance. As he had it now in his power to observe the phenomena that attended its formation, he discovered, and has communicated to the world, a method of producing it, which never fails. Since his late paper upon the subject, as the necessary precautions can be followed by every chemist, Mess. CLEMENT and DESORMES have obtained that credit to which their experiments had, in truth, always been entitled; and the formation, of what Professor LAMPADIUS terms his sulphur-alcohol is no longer a result of chance, or accounted for by being supposed one of those subterfuges to which human pride resorts, in order to spare itself the confession of human weakness.

The observation of any new fact becomes a matter of general concern, and truly worthy of philosophic contemplation, then only, when its influence is likely to be extended beyond the single instance to which it owes its discovery. Whether water were a simple body or a compound could have been of little importance as an insulated fact; but, connected with the vast chain of reasoning it gave rise to, it opened a new field for genius to explore. If in the present case our researches were to be confined merely to ascertaining whether palladium were a simple metal or a compound, all the advantages likely to arise from the facts observed during the inquiry would be lost; and an object of the most comprehensive interest would thus sink into a controversy concerning the existence of one more of those substances, which we have dignified with the name of elements. It was in this point of view that Mess. RICHTER and RITTER considered the subject as far as they went, and a few facts are stated in my first Paper in support of the opinion, that palladium is but a particular instance of a general truth.

By taking the reasoning on this subject then, in its widest extent, we shall be led, I think, to the following conclusion: That metals may exercise an action upon each other, even in their metallic state, capable of so altering some of their principal properties as to render the presence of one or more of them not to be detected by the usual methods. In this is contained the possibility of a compound metal appearing to be simple; but to prove this must be a work of great time and perseverance; and can only be done by considering singly and successively the different cases which it contains, and by instituting experiments upon each. When an affinity which unites two bodies, and so blends their different properties as to make them apparently one, has taken its full effect, it will not be easy to separate them; and this will be more particularly the case when neither of those substances is remarkable for exercising a powerful action upon others. The method of analysis therefore does not promise much success; and the labour of synthesis is sufficient to deter any individual from the undertaking.

It is my intention now to exhibit one example of my position, and to prove that platina and mercury act upon each other, in such a manner as to disguise the properties of both. I shall therefore wave for the present all consideration of palladium, which is in fact but a subordinate instance of the case before us.

When a solution of green sulphate of iron is poured into a solution of platina, no precipitate, nor any other sensible change ensues. This I had already observed, and it has since been confirmed by all who have written upon the subject. But, if a solution of silver or of mercury be added, a copious precipitate takes place. This precipitate contains metallic platina and metallic silver or mercury; some muriate of one or other of the latter metals is also present, as it is not easy to free the solution of platina from all superfluous muriatic acid. But these salts are of no importance in the experiment, and can be separated by such methods as a knowledge of their chemical properties will easily suggest. The proper object of consideration is the reduction of the platina to the metallic state, which does not happen when it is alone. I have tried to produce the same effect with other metals and platina, but I have not observed any thing similar. It is therefore fair to conclude, that when a solution of platina is precipitated in a metallic state by a solution of green sulphate of iron, either silver or mercury is present.

The precipitation of a mixed solution of platina and silver requires no further caution than to free the salt of platina as much as possible from muriatic acid; for as I observed in my former Paper, the effect of nitrate of silver poured into muriate of platina, is to produce a precipitate, not of muriate of silver, but of a triple muriate of platina and silver. It was by this experiment that I then proved the affinity of these two metals; for when silver is not present, muriate of platina is among the most soluble salts. The best method of presenting the three solutions of platina, silver, and green sulphate of iron to each other, is first to pour the filtered solution of the last into the solution of platina, and then, after mixing them thoroughly together, to add the solution of silver by degrees, and to stir them constantly. In this, as in all similar operations, the presence of all acids, salts, &c. excepting those necessary for the operation, should be avoided; and if proper proportions have been used, and all circumstances attended to, the precipitation of these two metals will be very complete.

But the precipitation by a solution of mercury requires to be further considered, as the state of oxidizement of this metal, as well as the acid in which it is dissolved, produces a considerable modification in the result. In the first place the oxide, at the minimum of oxidizement, dissolved in muriatic acid, is unfit for the experiment; and even the red oxide dissolved in the same acid, or corrosive sublimate, is not the most advantageous. When a warm solution of the latter is poured into a mixed solution of platina and green sulphate of iron also warm, as in the case of silver, these substances are brought into contact under the most favourable circumstances. Yet even thus the precipitation is slowly and imperfectly formed, often not till several hours have elapsed; and sometimes a very great deficiency of weight is observed, between the quantities used and those recovered directly by this method. If a solution of nitrate of mercury be used, the effect is produced more rapidly, and the precipitate is more abundant. The precipitation of muriate of platina by nitrate of silver, and the combination which ensues from it, suggested to me an experiment which I must state at length, as from the result of it consequences are deduced which modify some of the experiments of my former Paper.

It occurred to me that a method of uniting platina and mercury without the intervention of any other metal, or of any substance but the solvents of these metals might be accomplished as in the case of silver and platina. I therefore poured a solution of nitrate of mercury, which solution being at the minimum of oxidizement, consequently formed an insoluble muriate with muriatic acid, into a solution of muriate of platina. The result was a triple salt of platina and mercury, which when the mercury was completely and totally at the minimum of oxidizement was nearly insoluble. To procure it in this state it is sufficient to put more metallic mercury into dilute nitric acid than the nitric acid can dissolve, and to boil them together. This triple salt of platina and mercury shall be presently examined. From this it is evident that to produce the union of platina and mercury, the latter being at its minimum of oxidizement in nitric acid the addition of green sulphate of iron is superfluous.

But if mercury be raised to its maximum of oxidizement in nitric acid the case is different, for no precipitation occurs till the green sulphate of iron is added. The most advantageous method for precipitating platina and mercury by green sulphate of iron is, I believe, the following. Mix a solution of platina with a solution of green sulphate of iron, both warm, and add to them a solution of nitrate of mercury at the maximum of oxidizement also warm. It is necessary to avoid excess of acid, salt, &c. in this as in all such cases. With due care the precipitation of both metals will then be complete.

By comparing the experiments made with mercury and platina with those made with silver and platina, a striking resemblance will be found. This induced me to pursue the analogy, and to examine whether, independently of the action of platina, mercury had not the same property of being precipitated by green sulphate of iron as silver. Nitrate of silver is precipitated by green sulphate of iron, but muriate of silver is not sensibly acted upon by the same reagent. The insolubility of muriate of silver might be alleged as the cause of this, if I had not tried the experiment by pouring nitrate of silver into green muriate of iron, in which case all the substances were presented to each other in solution. The result was not reduction, but muriate of silver and nitrate of iron. This fact rests upon a much more extensive basis than mere mechanical circumstances; and, if pursued with the attention it deserves, it would lead us into the wide expanse of complicated affinities and their relations. From reasoning alone we should be disposed to think that an acid, so easily decomposed as the nitric, would be sufficient to prevent the reduction of a metal which it can dissolve. But on the one hand it can spend its oxygen upon a part of the oxide of the green sulphate of iron, while on the other its affinity for oxide of silver is not powerful enough to retain it, when there is another part of the oxide of iron present to deprive it of oxygen. But the affinity of muriatic acid for oxide of silver, one of the strongest at present known, is sufficient to counterbalance all the other forces. There are many other instances of the same kind.

If then a solution of green sulphate of iron be brought into contact with either soluble or insoluble muriate of mercury, no reduction takes place; but if mercury, whether at the maximum or the minimum of oxidizement, be dissolved in nitric acid, and green sulphate of iron be added, the mercury is precipitated in the metallic state.

These experiments are much stronger examples than the former of the effects produced by complicated affinities. They are of importance not only as objects of general consideration but in their application to the present subject. They most materially modify and are indispensable to the accuracy of the results I formerly stated; but I was not aware of them at the time I first engaged in the investigation of this subject. I can also now explain a very material difference between some proportions observed by M. Richter and myself in an experiment which that chemist had made as a repetition of one of mine.

I had poured a solution of green sulphate of iron into a solution of 100 parts of gold and 1200 of mercury, and had obtained a precipitate consisting of 100 of gold and 774 of mercury. M. Richter repeated, as he terms it, this experiment; that is, he used 100 of gold and 300 of mercury, and obtained a precipitate weighing 102. He is surprised at the difference of weight between our results, which might be owing to his *method* of *repeating* the experiment; but the real cause of this difference lies, as I suppose, in my having accidentally used nitrate instead of muriate of mercury. I had never observed that with mercury and silver this operation had failed, and it must have been, because, on account of the known effect of muriatic salts upon those of silver, I had naturally avoided using a muriate of mercury.

But the state of the nitrate of mercury which is used with a solution of gold is not indifferent. As green sulphate of iron reduces mercury when dissolved in nitric acid, as well as gold, it is necessary to mix the solutions of those metals before the green sulphate of iron is added, in order that both may be acted upon together. If the nitrate be at the minimum of oxidizement, a precipitate is immediately formed upon mixing the solutions of gold and mercury. Calomel is produced by the muriatic acid of the solution of gold and the oxide of mercury; whilst the gold is reduced to the metallic state by a portion of the oxide of mercury becoming more oxidized, and forming the soluble muriate. The precipitate consists of calomel, of metallic gold, and of a very small portion of mercury which I believe to be in the same state; my reason for thinking so, is, that I have often observed, that a glass vessel in which I had sublimed some of it, was lined with a thin gray metallic coat. If, on the contrary, a nitrate of mercury be highly oxidized, no precipitate nor reduction of gold takes place until the green sulphate of iron is added. But at any rate the precipitation of gold and mercury, or of silver and mercury by green sulphate of iron cannot be adduced as an argument to support the affinity of these metals, since the effect is the same, whether they are separate or united.

These preliminary considerations were necessary as well for the rectification of my former experiments as for the pursuit of my present object; and now to return to platina.

Exper. 1. If a solution of highly oxidized nitrate of mercury be poured into a mixed solution of platina and green sulphate of iron, the first action which takes place passes between the muriatic acid of the solution of platina and the oxide of mercury, by which a muriate of mercury is formed, but retained in solution. This effect makes it advantageous to use a greater quantity of the solution of mercury than is merely capable of drawing down the given quantity of platina along with itself in the form of a metallic precipitate. When this precipitate is washed and dried, it will be found to weigh much more than the original quantity of platina; and the augmentation of weight has no limit but those of the mercury and the green sulphate of iron employed. But even after nitric acid has been boiled for a long time and in great quantities upon this precipitate, until it no longer dissolves any part of it, there still remains more undissolved matter than the original weight of the platina used in the experiment. By exposure to heat little more is left in general than the original platina; and sometimes even a diminution may be observed; for as the experiment is not attended with uniform success, it does not always happen that the whole of the platina is precipitated, but a portion of it will sometimes resist the action of the green sulphate of iron, even when sufficient mercury has been used. Before the precipitate has been exposed to heat it is dissolved more easily than platina by nitro-muriatic acid; and the solution when nearly in a neutral state gives a copious metallic precipitate, (yet not equal to the quantity employed,) when boiled with a solution of green sulphate of iron.

Exper. 2. When a mixed solution of platina and mercury is precipitated by metallic iron, a quantity equal to the sum of the former metals is generally obtained. After nitric acid has been boiled for a long time upon the precipitate so formed, the original weight of platina, together with a considerable increase, remains behind, nor can nitric acid sensibly diminish it. It yields more easily than platina to the action of nitro-muriatic acid, and its solution in that acid, when neutralized, gives a precipitate, as in the former experiment, by green sulphate of iron. If this precipitate be exposed to a strong heat after it has been boiled with nitric acid, it loses a great part of its weight, and the platina alone will generally be found to remain.

Exper. 3. When a quantity of ammoniacal muriate of platina is treated according to the method of Count MUSSIN PUSHKIN to form an amalgam, and, after being rubbed for a considerable time with mercury, is exposed in a crucible to a heat gradually increased till it becomes violent, a metallic powder remains in the crucible. This powder is acted upon by nitro-muriatic acid, and when the solution is neutralized, a copious precipitate is formed upon the addition of green sulphate of iron. This effect takes place even after the metal has been fused in the manner described in the former part of this Paper.

Exper. 4. If sulphur be added to the ingredients recommended by Count MUSSIN PUSHKIN, and the whole treated as in the last experiment, the quantity of precipitate caused by green sulphate of iron in the nitro-muriatic solution of the button which results from the operation, is generally more considerable.

Exper. 5. If sulphur be rubbed for some time with ammoniacal muriate of platina, and the mixture be introduced into a small Florence flask, it can be melted on a sand-bath. If mercury be then thrown into it, and the whole be well stirred together and heated, it may afterwards be exposed to a very strong fire and melted into a button. If this be dissolved in nitro-muriatic acid, it will give a precipitate, as in the former cases, by green sulphate of iron.

Exper. 6. If a current of sulphuretted hydrogen gas be sent through a mixed solution of platina and mercury, and the precipitate which ensues be collected, the metal may be reduced by heat; and with the addition of borax, it may be melted into a button which will not contain any sulphur. Green sulphate of iron causes a precipitate in the solution of this metal also.

Exper. 7. If to a mixed solution of platina and mercury, phosphate of ammonia be added, a precipitate takes place. If this be collected and reduced, it will be acted upon by green sulphate of iron poured into its solution, in the same manner as the metallic buttons in the preceding examples.

Exper. 8. I have already mentioned that when a solution of nitrate of mercury, at the minimum of oxidizement, is poured into a solution of muriate of platina, a mercurial muriate of platina is precipitated. The supernatant liquor may be decanted and the residuum washed; if this be reduced and afterwards dissolved in nitro-muriatic acid, it will yield a precipitate with green sulphate of iron. This method appears to me to be the neatest for combining platina and mercury, as the action which takes yet in this state it can draw down a greater quantity of the latter, than when it is merely poured into a mixed solution of platina, not before so treated. Indeed the whole of these experiments tend, not only to show that these two metals exercise a very powerful action upon each other, but that they are capable of great variation in the state of their combination; and also that substances possessing different properties have resulted from my attempts to combine platina with mercury.

This observation furnished me with a method of ascertaining, or at least of approaching to the knowledge of, the quantity of mercury thus fixed by platina, and in combination with it. The experiment, however, having been seldom attended with full success, I mention the result with the entire consciousness of the uncertainty to which it is subject. I observed the increase of weight, which the original quantity of platina had acquired in some cases after it had been treated with mercury, and fused into a button. I counted that augmentation as the quantity of mercury fixed. I then determined how much was precipitated by green sulphate of iron from a solution of this alloy, and supposed it to contain the whole quantity of mercury found as above. But, even if attended with complete success, there is a chemical reason which must make us refuse our assent to this estimate. It is possible, and not unlikely, that a portion of mercury may be retained in solution by the platina, as well as that a portion of the platina may be precipitated by means of the mercury. The mean result, however, was that the precipitate by green sulphate of iron consisted of about 17 of mercury, and 83 of platina, when the specific gravity was about 16.

With regard to palladium, lest it should be supposed that either my own observations, or those of others have given me cause to alter my opinion. I will add that I have as yet seen no arguments of sufficient weight to convince me, in opposition to experiment, that palladium is a simple substance. Repeated failure in the attempt to form it I am too well accustomed to, not to believe that it may happen in well conducted operations; but four successful trials, which were not performed in secret, are in my mind a sufficient answer to that objection. By determining the present question we may overcome the prepossession conceived by many against the possibility of rendering mercury as fixed, at an elevated temperature, as other metals: we may be led to see no greater miracle in this compound than in a metallic oxide, or in water, and be compelled to take a middle path between the visions of alchemy on the one hand, and the equally unphilosophical prejudices on the other, which they are likely to create. In the course of experiments just now related, I have seen nothing but what tends to confirm my former results, yet the only means which I can, after all, prescribe for succeeding, is perseverance.

To ascertain whether the opinion of Mess. FOURCROY and VAUQUELIN, that the new metal was the principal ingredient in palladium had any just foundation, I observed the methods they have recommended for obtaining pure platina; but I did not perceive any difference in the facility with which either kind of platina combined with mercury.

I might have added some more experiments to corroborate the evidence I have adduced to prove my assertion of the fixation of mercury by platina; but Mess. VAUQUELIN and FOURCROY have promised the Institute of France a continuation of their researches, and M. RICHTER concludes his paper with saying that he will return to the subject. From the labours of such persons some great and important fact must issue, and I hope that the present subject will not be excluded from their consideration. The facts contained in this Paper cannot be submitted to too severe a scrutiny; and no judge can be more rigid or more competent than the very person who was the first to doubt my former experiments. But it is necessary to be observed by whoever shall think them worth the trouble of verifying, that even these experiments are liable to fail unless proper precautions are used: that I have never operated upon less than one hundred grains; and that the results, which I have stated, however simple they may appear, have been the constant labour of some weeks.

POSTSCRIPT.

Since this Paper was written Dr. WOLLASTON has published some experiments upon platina. He has found that palladium is contained in very small quantities in crude platina. This fact was mentioned to me more than a year ago by Dr. WOLLASTON. I have not yet seen a copy of his Paper; but I shall merely observe here that, whatever be the quantity of palladium found in a natural state, no conclusion can be drawn as to its being simple or compound. Nothing is more probable than that nature may have formed this alloy, and formed it much better than we can do. At all events the amalgamation to which platina is submitted before it reaches Europe is sufficient to account for a small portion of palladium.

VI. *An Investigation of all the Changes of the variable Star in* Sobieski's *Shield, from five Year's Observations, exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures respecting unenlightened heavenly Bodies.*

By Edward Pigott, *Esq. In a Letter to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read February 7, 1805.

Bath, 1802.

THE object of the first part of this Paper is a further investigation of the periodical and other changes of brightness of one of the variable stars I discovered in 1795, that in SOBIESKI's *shield*, an account of which the Royal Society did me the honour of publishing in their Transactions. Those determinations being deduced from a few periods made *near the time of discovery*, must of course remain unsatisfactory, however exact the observations themselves may be, until *confirmed* by an additional set, or by others made at a greater interval of time; for which purpose I occasionally continued keeping a journal of its changes for near five years, and am happy to find that they have answered my expectation, particularly by giving us an insight into its irregularities, as will be shewn hereafter.

Variable Star in SOBIESKI's *Shield.*

$$\left.\begin{array}{lll}\text{R. A.} & - - & 279^\circ\ 9\tfrac{1}{2}' \\ \text{S. declination} & & 5^\circ\ 56'\end{array}\right\}\text{for the end of June, 1796.}$$

Its rotation on its axis was, in 1796, estimated at 62¼ days, from a mean of six observations of its greatest and least brightness. Here follow about 26 similar determinations, most of them the results of very accurate observations; and as they probably will in future be compared with others, I have examined them repeatedly with the utmost care, attending particularly to the progression of their changes.

Table I.

Dates when at its greatest Brightness.	Magnitudes.	Dates, when at its least Brightness.	Magnitudes.
1796. September 17	5	1796. September 3	6
November 13	5—	October 22 -	6
1797. May 14: -	5+	1797. July 10 - -	5.6
August 7 -	5	September 15	6
October 15 -	6.5	November 6	6
1798. July 29 -	5+	1798. July 10 - -	6
October 25 -	5.6	September 15	9.0
December 5 : :	5.6	November 10	6
1799. June 1 : : -	6.5	1799. July 4 -	7
August 7 -	5	September 16	6
October 11 -	5+	November 5 :	6.7
1801. July 14 : -	5	1801. June (middle):	6
September 24	5	August 21 -	6.7
		October 16 -	6.5

The + and — annexed to the magnitudes denote them to be more or less bright; the doubtful results are marked with dots; all the others are esteemed exact, except those of August 7, 1797, and August 21, 1801, which are in *a small degree* less so. From these determinations the rotation on its axis may be computed as follows.

Table II.

Middle of its greatest *Brightness.*

	Dates.	Interval in Days.				Number of Periods.
1796.	September 17 November 13	57	-	equal to	-	1
1797.	May 14 : August 7 -	85	-	=	-	1
	August 7 - October 15	69	-	=	-	1
1798.	July 29 - October 25	88	-	=	-	1
1799.	August 7 - October 11	65	-	=	-	1
1796. 1797.	November 13 May 14 : -	182 or 61—	- -	= =	- -	3 1
1796. 1797.	November 13 August 7 -	267 or 67—	- -	= =	- -	4 1
1797. 1798.	October 15 July 29 - -	287 or $57\frac{1}{2}$	- -	= =	- -	5 1
1798. 1799.	October 25 August 7 -	286 or 57+	- -	= =	- -	5 1
1801.	July 14 : - September 24	72 :	-	=	-	1

Table III.

Middle of its least Brightness.

	Dates.	Interval in Days.		Number of Periods.
1796.	September 3 October 22 -	49	- equal to -	1
1797.	July 10 - September 15	67	- = -	1
	September 15 November 6	52	- = -	1
1798.	July 10 - - September 15	67	- = -	1
	September 15 November 10	56	- = -	1
1799.	July 4 - September 16	74	- = -	1
	September 16 November 5:	50:	- = -	1
1801.	August 21 - October 16 -	56	- = -	1
1796. 1797.	October 22 - July 10 -	261 or 65+	- = - - = -	4 1
1797. 1798.	November 6 July 10 - -	246 or $61\frac{1}{2}$	- = - - = -	4 1
1798. 1799.	November 10 July 4 - -	236 or 59	- = - - = -	4 1

From all these results it appears, that the disagreements between them are far greater when at its full brightness than at its least; I shall therefore, in summing up the first set, omit two of them, as they evidently differ considerably from the others.

Table IV.

Rotation from Observations of its full Brightness. Days.	Rotation from Observations of its least Brightness. Days.
57	49
69	67
65	52
61—	67
67—	74
57½	50
57+	56
72	56
	65+
	61½
	59
by its full brightness 63+ on a mean.	By its least ditto 59¾ on a mean.

A mean of these two means being 61½ days, agrees with the first deductions to 1¼ day, a coincidence that certainly I could not flatter myself would have happened: yet it must be remembered, that the intervals with considerable perturbations were omitted; for, had they been included, the length of period resulting from its *maxima* of brightness would have varied much more from that obtained from its *minima*. I shall now proceed to examine some of its other changes.

Table V.

Decrease from the Middle of its full Brightness to the Middle of its least. See Table I.			Increase from the Middle of its least Brightness to the Middle of its full. See Table I.		
1796.	September 17 October 22	35 days.	1796.	September 3 September 17	14 days.
1797.	May 14: - July 10 -	57		October 22 November 13	22
	August 7 - September 15	39	1797.	July 10 - August 7 -	28
	October 15 - November 6	22		September 15 October 15	30
1798.	July 29 - September 15	48	1798.	July 10 - July 29 - -	19
	October 25 November 10	16		September 15 October 25	40
1799.	August 7 - September 16	40	1799.	July 4 - - August 7 -	34
	October 11 November 5	25		September 16 October 11	25
1801.	July 14 - August 21 -	38	1801.	August 21 - September 24	34
	September 24 October 16	22			
		34 on a mean.			27+ on a mean.

The sum of these two means (61+), agreeing so satisfactorily with the whole rotation ($61\frac{1}{2}$), no correction is requisite, as was the case with the former determinations of 1796 to reduce them to 28 and 35 days, results that differ considerably from the above (34 and 27+); but as they were deduced from only *two* intervals, the disagreement cannot be of any consequence, provided the number of each set be *proportionally* attended to in the computation, and then the mean of the whole will be 33+ and 29− days: thus it appears that the *time of the decrease is longer* than that of the *increase*, and consequently that the places of the full and the least brightness are not situated at the distance of half the circumference from each other: the like circumstance will be found to be the case with most, if not all, of the variable stars. The next particulars to be reviewed are *the durations* of its brightness without *any perceptible change*, while at its *maximum and minimum*. These determinations require a tolerable *succession* of observations; where therefore that is not the case, they are omitted.

Table VI.

Duration of Brightness at its Maximum.			Duration of Brightness at its Minimum.		
	Days.	Magnitudes.		Days.	Magnitudes.
1796.			1796.		
September 17	9	5	September 3	7	6
November 13	8	5—	October 22	8	6
1797.			1797.		
October 15	32	6.5	July 10	24	5.6
1798.			September 15	18:	6+
July 29	6	5+	November 6	6	6
October 25	10	5.6	1798.		
December 5	16 or more	5.6	July 10	12	6
1799.			September 15	9	9
June 1	16::	6.5	November 10	8	6+
August 7	8	5	1799.		
October 11	8	5+	July 4	9	7
1801.			September 16	10	6
September 24	15::	5	November 5	16:	6.7
			1801.		
			October 16	9	6.5

It appears in general by my journal, and from these results, that when *the degree* of brightness at its maximum is *less than usual*, and its minimum *not much decreased*, the changes take place but very slowly, and cannot be settled with much accuracy, unless the observations have been made frequently, and with great attention; therefore, in summing them up, I think four of the first set and three of the second may be omitted, and then the duration at its maximum will be on a mean 8 + days,
and ditto 20 — days
when it does not attain its usual brightness;
and at its minimum - - - on a mean 9 — days,
and ditto 20 — days
when its decrease is not so great as usual; the former observations make them 14 and 9 days.

Some of its *degrees* of brightness annexed to the results, have occasionally been noticed, as far as it was necessary, but the list of them I am going to give, is more exact and full. It will be there seen, that its brightness is seldom the same for two or three successive periods; that the change in half a rotation is sometimes from the 5th to the 7th magnitude, and sometimes only half a one or scarcely perceptible: its decrease has also been greater than by the former observations, particularly on September 15, 1798, and August 9, 1803,* when it was less than the 9th magnitude, or had even disappeared.

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* Added since the Paper was written.

Table VII.

Dates.	Magnitudes when at its full Brightness.	Dates.	Magnitudes when at its least Brightness.
1796. September 17	5	1796. September 3	6
November 13	5 small	October 22 -	6
1797. May (middle)	5 bright	1797. July 10 -	5.6
August 7 -	5	September 15	6 bright
October 15	6.5	November 6	6
1798. July 29 -	5 bright	1798. July 10 -	6
October 25	5.6	September 15	9 or 0
December 5	5.6	November 10	6 bright
1799. June 1 -	6.5	1799. July 4 -	7
August 7 -	5	September 16	6
October 11 -	5 bright	November 5	6.7
1801. July 14 -	5	1801. June (middle)	6
September 24	5	August 21	6.7
		October 16	6.5
		1803. August 9* -	9 or 0

In concluding these determinations I shall collect together, as follows, in one view, all the different changes that have been examined; the first column describes them, the second exhibits the present results, the third the former ones, and the last column a mean of both, computed *proportionally* according to the number of observations of each.

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* Added since the Paper was written.

Table VIII.

	Days.	Days.	Days. on a mean.
Rotation on its axis - - -	61½	62¾	62—
Duration of brightness, at its maximum, without any perceptible change -	8+	14	9½
Ditto, when it does not attain its usual brightness - - - -	20—	—	—
Duration of brightness at its minimum, without any perceptible change -	9—	9	9
Ditto, when it does not decrease so much as usual - - - -	20—	—	—
Decrease in time, from the middle of its full brightness to the middle of its least - - - - -	34	28	33+
Increase in time, from the middle of its least brightness to the middle of its full - - - -	27+	35	29—
Extremes of its different degrees of brightness; with a mean of its usual variations - - - -	5+ 9 or 0	5+ 7.8	5. 6

SECOND PART.

Fontainbleau, 1803.

These essential variations of the star being thus settled with considerable precision, we may proceed to examine some of its other phenomena, particularly one common to most of the variables, as likewise in some degree to our sun, *viz. that the times of their periodical returns of brightness* are, in general, IRREGULAR, a circumstance I apprehend sufficiently interesting to engage our attention, at least I have ever thought so, and was thereby induced a few years past to make a succession of observations on one of them, in hopes of finding in what manner such *irregularities* took place, or at least to leave to future astronomers determinations, that might lead them to form some ultimate opinion thereon. I therefore chose for that purpose the star in SOBIESKI'S *shield,* on account of the time of its revolution on its axis being comparatively of a moderate length, *viz.* 62 days, and shall here have the honour of laying before the Society the appearances that occurred, point out the various results deduced from the observations, and attempt to explain them. The two following Tables are the observed middle times of its *full* and *least* brightness, with deductions of the star's apparent rotation from *single intervals,* which in the present examination can alone be admitted, because a mean taken of two or several would in general make such irregularities disappear, by the long and the short ones compensating each other. The remarks for the present need not be attended to, as they are chiefly to explain the reliance that may be put on some of the observations.

Table IX.

The observed middle Times of its *full* Brightness.	Apparent Rotations in Days.	REMARKS, chiefly to illustrate some of the Observations.
1795. October 1 -	70	By the observations of November, &c. it seems probable it had not obtained its full brightness before December 10, although possibly much later.
December 10:		
1796. April 10 -	69	
June 18 - -	39	The increase towards July 27 was so slight that I had much hesitation in adopting it as a full brightness; if omitted, the interval will be 91 days. See Phil Trans. 1797.
July 27 -	52	
September 17 -	57	
November 13		
1797. May 14: -	85	The full brightness in May is doubtful to only about 6 days; the observations afterwards, to August the 7th, were made with tolerable regularity.
August 7 -	69	
October 15 -		
1798. July 29 -	88	A regular succession of observations were made between July and October 25.
October 25 -	41	The last observation made, was on December 10, when it shewed no appearance of decreasing, although it had been 16 days at its full brightness.
December 5:		
1799. June 1: -	67	The full brightness lasted a fortnight.
August 7 -	65	
October 11 -		
1801. July 14: -	72	The observation of July 14, is doubtful to a few days, to which perhaps the excess may be attributed.
September 24	38	A regular succession of observations were made between September and the middle of November. This last determination was deduced after the first part of this Paper was finished.
November 1		

Table X.

The observed middle Times of its *least* Brightness.	Apparent Rotations in Days.	REMARKS, chiefly to illustrate some of the Observations.
1796. March 4 -	67	
May 10 -	70	The decrease of July 19 being so very slight, I for a long time omitted it, and took the interval from May to September of 116 days as a double revolution, but have here preferred the separate ones of 70 and 46 days. See Phil. Trans. 1797.
July 19 -	46	
September 3	49	
October 22 -		
1797. July 10 -	67	
September 15	52	
November 6		
1798. July 10 -	67	
September 15	56	
November 10		
1799. July 4 -	74	The increase and decrease observed by a succession of good observations.
September 16	50	
November 5 : -		
1801. Middle of June : :		
August 21 -	56	
October 16 -		

It thus appears, that the periodical returns of brightness are uncommonly fluctuating, and that the differences between the extremes are very considerable; to account for which, I shall presume to offer the following explanations, suggesting previously a few plausible conjectures, and some inferences arising from the observations themselves.

1st. That the body of the stars are dark and solid.

2d. Their real rotations on their axes are regular.

3d. That the surrounding medium is by times generating and absorbing its luminous particles in a manner nearly similar to what has been lately so ingeniously illustrated by the great investigator of the heavens, Dr. Herschel, with regard to the sun's atmosphere.

4th. That these luminous particles are but *sparingly dispersed* in the atmosphere surrounding the variable star of Sobieski, appears from the star being occasionally diminished to the 6.7 magnitude, and much less. July 4, 1799, it was of the 7th; September 15, 1798, and August 9, 1803, *of the 9th, if not invisible.* (See Table VII.) Does not this indicate a very small portion of light on its *darkened hemisphere?*

5th. And may we not with much plausibility consider them as spots, somewhat circular, or of no great extent? for even on its *brightest hemisphere* the *duration* of its full lustre is, on a mean, only 9½ days of the 62, or about one-sixth and ½ of its circumference. (See Table VIII. page 140.) The dimensions therefore of the parts enlightened seem much circumscribed, and can be tolerably estimated, and consequently may be represented very small, particularly if the *powerful effect of a little light* and *the length of time* a bright spot is remaining in view be taken into consideration.

6th. And a further ground of presumption that those principal bright parts are but slight patches is, that they undergo *perpetual changes*, and also that such changes are very visible to us, for most probably they would be imperceptible, were not the bright parts contrasted by considerable intervals or diminutions of light.

7th, and last. We may obtain some idea of the *relative situation or intervals between* these bright parts, by the observations of the increase and decrease of brightness, as thereby the changes and times elapsed are pointed out. (See Table V. page 136; and Phil. Trans. for 1797.)

I have tried practically the effect of the above suppositions, by placing small white spots on a dark sphere, which being revolved round represented the various changes as nearly as could be expected: proceeding therefore with these and other considerations, I shall make ideal drawings of the star with the small illuminated parts in its atmosphere, and apply to them some of the actual observations from both the preceding Tables, having always in view that each period may, more or less, require a different disposition of spots, in consequence of their constant changeability.

1*st View.*

Plate II. Fig. 1, A B, the star's polar axis, round which its rotation takes place in 62 days from C to D.

CD, its equator, the 360 degrees of which being revolved in 62 days, gives nearly 5¾ degrees for each day's motion; the brightest part or spot is represented as centrally facing us, and accordingly shewing the star in its greatest lustre. Were this bright spot and the other parts to remain *unchangeable* they would after having completed the revolution of 360 degrees or 62 days, (the star's rotation on its axis,) appear again as at first, and at every return continue to give exact periodical times, as was nearly the case in 1799 between August and October, (See Table IX. p. 142,) but if the spot becomes obscure and another brightens up in a different place, this latter will make the star appear at its next full splendour either sooner or later than the real rotation according to its position, thus,

2d View.

Fig. 1. A full brightness having been shewn by the same spot, it afterwards loses its light and another as bright is produced 5 days motion (or 29 degrees) preceding it at E, see Fig. 2. This latter, when turned centrally to the earth, will appear 5 *days sooner* than the former one, now obscured, (here marked P,) and show the star at its full lustre, making the rotation 57 days instead of 62, which was the case in 1796, the observed revolution between September 17 and November 13. (See Table IX.)

3d View.

Fig. 3. We will now apply a case of an interval of too great length, that of 72 days: the spot *m* alone having shewn us the star in its full lustre, its light disappears during the revolution, and another brightens forth ten days (or 58 degrees) *following it* at H; when *m* returns to face us again in 62 days it being obliterated, the star will still appear obscured, and not recover its splendour until the new brightened part H becomes central, which being *ten days later* than the position in which *m* was seen, makes the revolution 72 days instead of 62, as was observed between July 14 and September 24, 1801. (See Table IX.) In the above case the alterations took place while behind the star, otherwise some irregularities would have been perceived, as will later be noticed. The same reasoning with proper alterations will, I apprehend, account for the other revolutions, yet I shall soon again resume the subject with

regard to a *series* of the greatest irregularities; at present let us proceed to take a few views of the intervals of its *least brightness*, which, contrary to my expectation, I find much more difficult to explain than those of the full, although the results disagree less among themselves. The darkened face of the star is here represented with a few small changeable bright spots, placed in general, at a proper distance, so as to keep up an uninterrupted increase and decrease of light with regard to us, and are also made to correspond with several other observations.

4th View.

Fig. 4 is to explain the greatest interval of 74 days, between July 4th, and September 16th 1799. (See Table X.) The darkened hemisphere here exhibited is its *minimum* July 4th, with the following spots, *w* nearly gone off, next a small one *l*, then another *P* of a similar size, preceding the centre a day or two, (or a few degrees,) and lastly a bright one at D, just appearing. During the rotation, D losing its light and the P becoming *much brighter*, the star at its next return in 62 days, when at its first position, must of course appear much brighter, (See fig. 5) but by the retiring of *l* and P continues to diminish in lustre till the appearance of some large spot from the other hemisphere; which taking place 12 days afterwards, will, (when this time is added to the 62 already revolved) make the revolution of 74 days, as required; for a view of a short interval, for the present let that of 56 days be taken, between August 21st and October 16th 1801. (See Table X.)

5th View.

The least brightness or *minimum* is represented by fig. 6, when the bright spots *y* and *x* at each extremity of the equatorial diameter are mutually but just in sight and a minute one, *r* alone on its surface preceding *y* by 6 days motion: *n n*, are other middling sized spots near *x*, but preceding it; they cannot for the present be seen, being on the opposite or bright hemisphere. The spot *x* during the stars revolution having lost its light, and *r* being considerably increased, the next *minimum* will be between *n n* and *r*, (instead of *x* and *y*.) See fig. 7; and by the retiring of *n n* the *diminution* of the star's light will continue to take place only until the reappearance of *r*, at the place where *y* was, which being 6 days sooner than the former position, (See fig. 6,) reduces the rotation to 56 days. All the foregoing views are from unconnected periods, where only the ultimate returns of each appearance have been attended to; but now, I shall examine a long interval with many intermediate changes, that between June 18th, and September 17th 1796, wherein are included the most intricate irregularities and vicissitudes: these observations are already printed at full length in the Philosophical Transactions for 1797, and therefore can at any time be inspected: indeed, I then little thought they would ever become of further use, but that of stating facts, to which, however, I have always been very partial, and particularly so, after having experienced the advantage of Maraldi's printed observations on the variable star in Hydra, as it was partly by them that I ascertained the periodical returns of brightness of that star, and which flattered me the more, as Maraldi himself had been less successful in the attempt; See Phil. Trans. for 1786. Yet in the present Paper I have omitted all such details, being aware they might be thought too voluminous, but hope at some future time the Society will honour them with a place in their library.

The first sketch, Plate III. represents, for June 13, 1796, the comparative size of the bright spots supposed to surround the star, but here extended at full length; the next eight following are spherical views, on an enlarged scale, for each quarterly rotation or less, shewing the principal changes, as expressed in the adjoining remarks, and corresponding with the observations; these being taken from my printed paper, as already mentioned, are marked in italics. It will be seen that the spots by which the changes are principally regulated, are placed at equal distances, yet intermediate ones might also frequently be inserted without occasioning any objection, but that of rendering the explanations more complex.

REMARKS ON PLATE III.

Fig. 2. "*June* 18*th. Full brightness Mag. bright* 5*th,*" before or after which date the star would appear less bright, by the spot E being removed from the centre, and one of the others out of view.

Fig. 3. "*July* 3*d,* 15 *days or* $\frac{1}{4}$ *rotation being elapsed since June* 18*th,* 5*th Mag. a little decreased*" by the removal of the brightest spot E, the *h* being much less.

Fig. 4. "*July* 19*th,* 16 *days or* $\frac{1}{4}$ *rotation* 5.6 *Mag. still decreased,*" N being much less than *h,* now gone off. *A slight minimum.*"

Fig. 5. "*July* 27*th,* 8 *days of the rotation,* 5 *Mag. rather* *increased*" by the considerable increase of N since four days, with the addition of F, *a slight full brightness.*

Fig. 6. "*Aug. 3d, 7 days of rotation, 5.6 Mag. decreased* by the going off of N, the E, which is now reappearing, being reduced to much less than F.

Fig. 7. "*Aug. 19th, 16 days or* $\frac{1}{4}$ *rotation, 5.6 Mag. again decreased,*" by the removal of F, by E being much less, and by the *h* also being considerably diminished.

Fig. 8. "*Sept. 3d, 15 days or* $\frac{1}{4}$ *rotation, 6 Mag. still more decreased,*" by the *h* being much less than E, which is now going off, and N scarcely reappearing, *another minimum.*

Fig. 9. "*Sept. 17th, 14 days or near* $\frac{1}{4}$ *rotation. 5 Mag. full brightness considerably increased,*" by N having retained its increased brightness of July 27, and now facing us centrally.

1st, Thus are exhibited, the two short intervals of its full brightness, one between June 18 and July 27, of 39 days, and the other between July 27 and Sept. 17, of 52 days. See Table IX.

2dly, The interval of 46 days between the two *minima* of July 19 and Sept. 3; See Table X.

3dly, The long decrease of 38 days between July 27 and Sept. 3, and

4thly, The rapid increases of 3 and 14 days between the 19 and 27th of July, and the 3d and 17th of September,

As also the other intermediate changes, yet I must again repeat, particularly as a few days error may occasionally proceed from the observations, that by these sketches it is not meant to give exact drawings of the size, distances or alterations of the spots, but merely to shew how the changes may take place, as, I believe, nothing of the kind has hitherto been offered to the public, either with or without corroborating observations; nor do I presume to think, that the explanations are the only ones or best that can be imagined, the more so, as they solely refer (for greater simplicity) to the star's equator, while possibly, were the spots placed in a northern or southern latitude, or permanent ones near the poles, or were a proper inclination, given to the polar axis, they might be more satisfactory: however, the materials themselves, the *observations* and *deductions* will I flatter myself ever be acceptable, and contribute to facilitate future conjectures, which from an allowable analogy may extend to similar parts of the starry system, with regard to the probability of establishing whether any of the most *irregular or particular* changes may not *return at fixt periods*, or after a certain number of rotations. I think we can entertain but slight hopes of it, owing to the *great fluctuation* of the luminous matter, as shewn by the *perpetual varying* of the *apparent* revolutions, magnitudes &c. See Tab. IX. X. and VII. Still it is natural to suppose, that some parts of the atmosphere of this star may have a less tendency than others to become luminous, so as to promote at different times, similar appearances; and indeed this is strongly indicated by the *intervals* of the *minima* being far *more regular* than those of the *full brightness*, which, with other reasons induce us to suspect that even one of its hemispheres is less favourably constituted or qualified, than the other for the generating of these particles, although they do occasionally encroach on both sides, as appears by the observations between June and August, See Phil. Trans. for 1797, or the eight sketches of 1796, and likewise in 1797, see Tab. VII. when during *three months* it was only reduced to the 5 or 6 Mag. by which the degree of brightness that surrounded it, must have been nearly equal: had the causes of varying its light then ceased, it would ever have continued to appear as an unchangeable star of the 5 or 6 Mag. and such is the case of several others that *formerly have been variables*, but for many years retain a steady brightness, as β Geminorum, δ Ursæ majoris, α Draconis, and perhaps that in the Swan's breast, while others, after *shewing their changes*, have *entirely disappeared*, owing to a total absorption of light, as the famous one in Cassiopea, in Serpentarius of 1604, that near the Swan's head, and doubtless many more. Does not this induce us to presume that there are also others, that have *never shewn* a glimpse of brightness? Lastly, *new variables* may become so at different periods, by an unusual and partial increase or diminution of their bright parts, as not unlikely was the case of ο Ceti, Algol α Herculis, &c. for these stars being by times very conspicuous, their changes, had they been always equally great, might have been easily noticed by the ancient astronomers, who observed only with the *naked eye*. A few lines above, I mentioned the probability that there existed *primary* invisible bodies or *unenlightened stars* (if I may be allowed the expression) that have ever remained in *eternal darkness;* how numerous these may be, can never be known. Would it then be too daring or visionary to suppose their numbers equal to those endowed with light? particularly when we take into contemplation the ample set of bodies visible only by reflected rays, that appertain to our own system, such as the planets, asteroides, comets, and satellites. Do not these, although but of a secondary nature, lead us to venture on the foregoing more enlarged conjecture; and moreover to suspect, that the *enlightened stars* are those that have already attained the highest degree of perfection? granting, therefore, such multitudes do really exist, clusters of them, by being collected together as in the milky-way, must intercept all more distant rays, and if free from any intervening lights, they would appear as *dark spaces* in the heavens, similar to what has been observed in the Southern Hemisphere. That so few of these obscure places are perceived, may be attributed to their being obliterated by the presence either of some scattered stars, or of other slight luminous appearances.

I have thus fully investigated the nature of this distant sun, a single one among many millions, and scarcely perceptible to the sight, yet of no less importance than our own grand luminary. But ours is still supplied abundantly with resplendent particles, while SOBIESKI'S variable star has them most sparingly dispersed over its sphere: a scantiness that apparently must occasion to its surrounding planets, constant vicissitudes of uncertain darkness, and repletion of light and heat. How far more enviable seems our situation! I mean that which we enjoy at present; there being strong reasons to believe, that the sun's luminous appearance has been at times considerably diminished; and I have little hesitation in conceiving that it may also be reduced at some future period to small patches, and then the apparent irregularities of its periodical rotations, which at present are only perceived by the observations of trifling dark spots, would become evidently conspicuous, particularly when seen at a distance as remote as the variable stars are from us. But such conjectural flights of fancy cannot too soon be dropt. I therefore shall conclude with observing, that these inquiries on the alterations of light of the stars have been so little discussed, that it is to be hoped they will not be discontinued; and although I have already troubled the Society with many papers concerning such changes, I nevertheless propose, ere long, having the honour of presenting them with one more, most probably my last, on this subject.

EDW. PIGOTT.

Fig. 1.

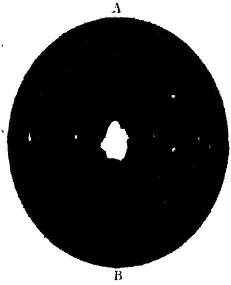


Fig. 2.

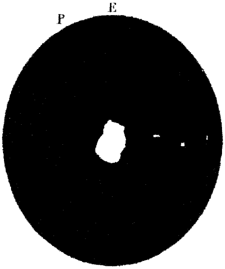


Fig. 3.

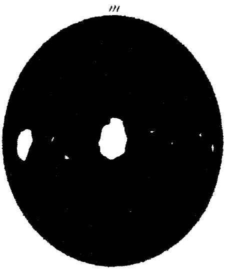


Fig. 4.

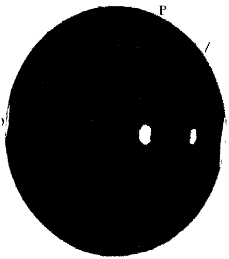


Fig. 5.

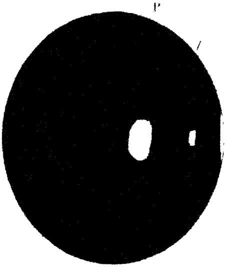


Fig. 6.

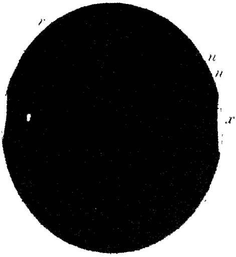
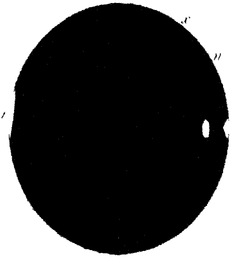
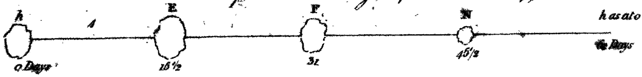


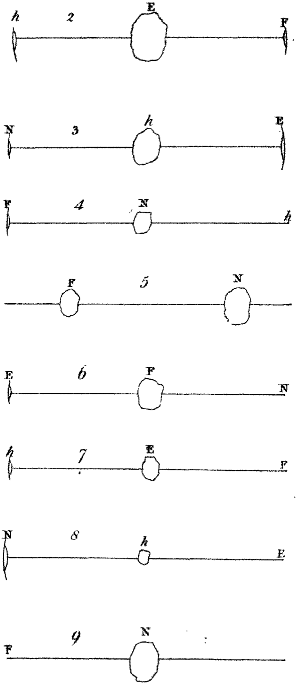
Fig. 7.



An extended View of the Surrounding Spots June 18th 1796.



Spherical Views



Basire sc.

VII. *An Account of some analytical Experiments on a mineral Production from Devonshire, consisting principally of Alumine and Water. By* Humphry Davy, *Esq. F. R. S. Professor of Chemistry in the Royal Institution.*

Read February 28, 1805.

I. *Preliminary Observations.*

THIS fossil was found many years ago by Dr. WAVEL, in a quarry near Barnstaple: Mr. HATCHETT, who visited the place in 1796, described it as filling some of the cavities and veins in a rock of soft argillaceous shist. When first made known, it was considered as a zeolite; Mr. HATCHETT, however, concluded, from its geological position, that it most probably did not belong to that class of stones; and Dr. BABINGTON, from its physical characters, and from some experiments on its solution in acids, made at his request by Mr. STOCKLER, ascertained that it was a mineral body, as yet not described, and that it contained a considerable proportion of aluminous earth.

It is to Dr. BABINGTON that I am obliged for the opportunity of making a general investigation of its chemical nature; and that gentleman liberally supplied me with specimens for analysis.

II. *Sensible Characters of the Fossil.*

The most common appearance of the fossil is in small hemispherical groups of crystals, composed of a number of filaments radiating from a common centre, and inserted on the surface of the shist; but in some instances it exists as a collection of irregularly disposed prisms forming small veins in the stone: as yet, I believe, no insulated or distinct crystal has been found. Its colour is white, in a few cases with a tinge of gray or of green, and in some pieces (apparently beginning to decompose) of yellow. Its lustre is silky; some of the specimens possess semi-transparency, but in general it is nearly opaque. Its texture is loose, but its small fragments possess great hardness, so as to scratch agate.

It produces no effect on the smell when breathed upon, has no taste, does not become electrical or phosphorescent by heat or friction, and does not adhere to the tongue till after it has been strongly ignited. It does not decrepitate before the flame of the blow-pipe; but it loses its hardness, and becomes quite opaque. In consequence of the minuteness of the portions in which it is found, few of them exceeding the size of a pea, it is very difficult to ascertain its specific gravity with any precision; but from several trials I am disposed to believe, that it does not exceed 2,70, that of water being considered as 1,00.

III. *Chemical Characters of the Fossil.*

The perfectly white and semi-transparent specimens of the fossil are soluble both in the mineral acids and in fixed alkaline lixivia by heat, without sensibly effervescing and without leaving any notable residuum; but a small part remains undissolved, when coloured or opaque specimens are exposed to the alkaline lixivia.

A small semi-transparent piece, acted on by the highest heat of an excellent forge, had its crystalline texture destroyed, and was rendered opaque; but it did not enter into fusion. After the experiment it adhered strongly to the tongue, and was found to have lost more than a fourth of its weight. Water and alcohol, whether hot or cold, had no effect on the fossil. When it was acted on by a heat of from 212° to 600° FAHRENHEIT in a glass tube, it gave out an elastic vapour, which when condensed appeared as a clear fluid possessing a slight empyreumatic smell, but no taste different from that of pure water.

The solution of the fossil in sulphuric acid, when evaporated sufficiently, deposited crystals which appeared in thin plates, and had all the properties of sulphate of alumine; and the solid matter, when redissolved and mixed with a little carbonate of potash, slowly deposited octahedral crystals of alum. The solid matter precipitated from the solution of the white and semi-transparent fossil in muriatic acid, was in no manner acted upon by solution of carbonate of ammonia, and therefore it could not contain any glucine or ittria; and its perfect solubility without residuum in alkaline lixivia shewed that it was alumine.

When the opaque *varieties* of the fossil were fully exposed to the agency of alkaline lixivia, the residuum never amounted to more than one-twentieth part of the weight of the *whole*. In the white opaque variety, it was merely calcareous earth, for when dissolved in muriatic acid, not in excess, it gave a white precipitate when mixed with solution of oxalate of ammonia, and did not affect solution of prussiate of potash and iron.

In the green opaque variety, calcareous earth was indicated by solution of oxalate of ammonia: and it contained oxide of manganese; for it was not precipitated by solution of ammonia; but was rendered turbid, and of a gray colour, by solution of prussiate of potash and iron.

The residuum of the alkaline solution of the yellow variety, when dissolved in muriatic acid, produced a small quantity of white solid matter when mixed with the solution of the oxalate of ammonia, and gave a light yellow precipitate by exposure to ammonia; but after this, when neutralized, it did not affect prussiate of potash and iron, so that its colouring matter, as there is every reason to believe, was *oxide of iron*.

IV. *Analysis of the Fossil.*

Eighty grains of the fossil consisting of the whitest and most transparent parts that could be obtained, were introduced into a small glass tube having a bulb of sufficient capacity to receive them with great ease. To the end of this tube, a small glass globe attached to another tube, communicating with a pneumatic mercurial apparatus, was joined by fusion by means of the blow-pipe.

The bulb of the tube was exposed to the heat of an ARGAND lamp; and the globe was preserved cool by being placed in a vessel of cold water. In consequence of this arrangement, the fluid disengaged by the heat, became condensed, and no elastic matter could be lost. The process was continued for half an hour, when the glass tube was quite red.

A very minute portion only of permanently elastic fluid passed into the pneumatic apparatus, and when examined, it proved to be common air. The quantity of clear fluid collected, when poured into another vessel, weighed 19 grains, but when the interior of the apparatus had been carefully wiped and dried, the whole loss indicated was 21 grains. The 19 grains of fluid had a faint smell, similar to that of burning peat; it was transparent, and tasted like distilled water; but it slightly reddened litmus paper. It produced no cloudiness in solutions of muriate of barytes, of acetite of lead, of nitrate of silver, or of sulphate of iron.

The 59 grains of solid matter were dissolved in diluted sulphuric acid, which left no residuum; and the solution was mixed with potash, in sufficient quantity to cause the alumine at first precipitated again to dissolve. What remained undissolved by potash, after being collected and properly washed, was heated strongly and weighed; its quantity was a grain and quarter. It was white, caustic to the taste, and had all the properties of lime.

The solution was mixed with nitric acid till it *became sour.* Solution of carbonate of ammonia was then poured into it till the effect of decomposition ceased. The whole thrown into a filtrating apparatus left solid matter, which when carefully washed and dried at the heat of ignition, weighed 56 grains. They were pure alumine: hence the general results of the experiments, when calculated upon, indicated for 100 parts of this specimen,

Of alumine - - -	70
Of lime - - -	1.4
Of fluid - - - -	26.2
Loss - - -	2.4

The loss I am inclined to attribute to some fluid remaining in the stone after the process of distillation; for I have found, from several experiments, that a red heat is not sufficient to expel all the matter capable of being volatilized, and that the full effect can only be produced by a strong white heat.

Fifty grains of a very transparent part of the fossil, by being exposed in a red heat for fifteen minutes, lost 13 grains; but when they were heated to whiteness, the deficiency amounted to 15 grains, and the case was similar in other trials.

Different specimens of the fossil were examined with great care, for the purpose of ascertaining whether any minute portion of fixed alkali existed in them; but no indications of this substance could be observed; the processes were conducted by means of solution of the unaltered fossil in nitric acid; the earths and oxides were precipitated from the solution by being boiled with carbonate of ammonia; and after their separation, the fluid was evaporated to dryness, and the nitrate of ammonia decomposed by heat, when no residuum occurred.

A comparative analysis of 30 grains of a very pellucid specimen was made by solution in lixivium of potash. This specimen lost 8 grains by long continued ignition, after which it easily dissolved in the lixivium by heat, leaving a residuum of a quarter of a grain only, which was red oxide of iron. The precipitate from the solution of potash, made by means of muriate of ammonia, weighed, when properly treated, 21 grains.

Several specimens were distilled in the manner above described, and in all cases the water collected had similar properties. The only test by which the presence of acid matter in it could be detected, was litmus paper; and in some cases the effect upon this substance was barely perceptible.

V. *General Observations.*

I have made several experiments with the hope of ascertaining the nature of the acid matter in the water; but from the impossibility of procuring any considerable quantity of the fossil, they have been wholly unsuccessful. It is, however, evident, from the experiments already detailed, that it is not one of the known mineral acids.

I am disposed to believe, from the minuteness of its proportion, and from the difference of this proportion in different cases, that it is not essential to the composition of the stone; and that, as well as the oxide of manganese, that of iron, and the lime it is only an accidental ingredient, and on this idea the pure matter of the fossil must be considered as a chemical combination *of about thirty parts of water and seventy of alumine.*

The experiments of M. THEODORE DE SAUSSURE on the precipitation* of alumine from its solutions, have demonstrated the affinity of this body for water; but as yet I believe no aluminous stone, except that which I have just described, has been found, containing so large a proportion of water, as thirty parts in the hundred.

The diaspore, which has been examined by M. VAUQUELIN, and which loses sixteen or seventeen parts in the hundred by ignition, and which contains nearly eighty of alumine, and only three of oxide of iron, is supposed by that excellent chemist to be a compound of alumine and water. Its physical and chemical characters differ however very much from those of the new fossil, and other researches are wanting to ascertain whether the part of it volatilized by heat is of the same kind.

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* Journal de Physique, Tom. LII. p. 280.

I have examined a fossil from near St. Austle, in Cornwall, very similar to the fossil from Barnstaple in all *its general chemical characters;* and I have been informed, that an analysis of it, made by the Rev. WILLIAM GREGOR some months since, proves that it consists of similar ingredients.

Dr. BABINGTON has proposed to call the fossil from Devonshire *Wavellite*, from Dr. WAVEL, the gentleman who discovered it; but if a name founded upon its chemical composition be preferred, it may be denominated *Hydrargillite*, from ὑδὼϱ water, and ἀϱγιλλος clay.

VIII. *Experiments on Wootz. By Mr.* David Mushet. *Communicated by the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read February 14, 1805.

THE following experiments were made at the request of Sir JOSEPH BANKS, on five cakes of wootz, with which he supplied me for that purpose. As the cakes, which were numbered 1, 2, 3, 4, 5, were not all of the same quality, it will be proper first to describe the differences observable in their external form and appearance.

No. 1 was a dense solid cake, without any flaw or fungous appearance upon the flat, or, what I suppose to be, the upper side. The round or under surface was covered with small pits or hollows, two of which were of considerable depth; one through which the slit or cut had run, and another nearly as large towards the edge of the cake. These depressions, the effects, as I suppose, of a species of crystallization in cooling, were continued round the edges, and even approached a little way upon the upper surface of the wootz.

The cake was a quarter of an inch thicker at one extremity of the diameter than at the other, from which I infer, that the pot or crucible, in which this cake had been made, had not occupied the furnace in a vertical position. Its convexity, compared to that of the other five, was second. Upon breaking the thin fin of steel, which connects the half cakes together, I found it to possess a very small dense white grain. This appearance never takes place but with steel of the best quality, and is less frequent in very high steel, though the quality be otherwise good.

Upon examining the break with attention, I perceived several laminæ and minute cells filled with rust, which in working are never expected to unite or shut together. The grain otherwise was uniformly regular in point of colour and size, and possessed a favourable appearance of steel.

No. 2. This cake had two very different aspects; one side was dense and regular, the other hollow, spongy, and protuberant. The under surface was more uniformly honey-combed than No. 1; the convexity in the middle was greater, but towards the edges, particularly on one side, it became flatter. The grain exposed by breaking was larger, bluer in colour, and more sparkling than No. 1. In breaking, the fracture tore but slightly out, and displayed the same unconnected laminæ with rusty surfaces, as were observed in No. 1. Beside these, two thin fins of malleable iron projected from the unsound side, and seemed incorporated with the mass of steel throughout. Towards the centre of the break, and near to the excrescence common to all the cakes, groups of malleable grains were distinctly visible. The same appearance, though in a slighter degree, manifested itself in various places throughout the break.

No. 3. The upper surface of this cake contained several deep pits, which seemed to result from the want of proper fluidity in fusion. They differed materially from those described upon the convex sides of No. 1 and 2, and were of that kind that would materially effect the steel in forging.

The under or convex side of this cake presented a few crystalline depressions, and those very small; the convexity was greater than that of No. 1 and 2, the fracture of the fin almost smooth, and only in one place exhibited a small degree of tenacity in the act of parting. In the middle of the break, about half an inch of soft steel was evident; and in different spots throughout numerous groups of malleable grains, and thin laminæ of soft blue tough iron made their appearance.

No. 4. Was a thick dense cake possessed of the greatest convexity; the depressions upon the under side were neither so large, nor so numerous as those in No. 1 and 2, nor did they approach the upper surface of the cake further than the acute edge. This surface had the most evident marks of hammering to depress the feeder, or fungous part of the metal, which in the manufacturing seems the gate or orifice by which the metal descends in the act of gravitation.

The break of this cake, however favourable as to external appearance, was far from being solid. Towards the feeder it seemed loose and crumbly, and much oxidated. The grain divided itself into two distinct strata, one of a dense whitish colour, the other large and bluish, containing a number of small specks of great brilliancy. Several irregular lines of malleable iron pervaded the mass in various places, which indicated a compound too heterogeneous for good steel.

5th cake. This was materially different in appearance from any of the former. It had received but little hammering, yet was smooth and free from depressions, or honey-comb on both surfaces. The feeder, instead of being an excrescence, presented a deep concave beautifully crystallized.

In breaking, the fracture tore out considerably, but presented a very irregular quality of grain. That towards the under surface was small and uniform, but towards the flat or upper surface it increased in size, and in the blueness of its colour, till it passed into the state of malleable iron.

The break of this steel, though apparently soft, was the least homogeneous of the whole, and throughout it presented a very brilliant arrangement of crystal, which in other steel is always viewed with suspicion.

General Remark.

Uniformly the grain and density of the wootz are homogeneous, and free from malleable iron towards the under or round surface; but always the reverse towards the feeder or upper side.

Remarks in Forging.

No. 1. One-half of the cake was heated slowly by an annealing heat to a deep red, and put under a sharp broad-mouthed chissel with a small degree of taper. It cut with difficulty, was reheated, and cracked a little towards one end of the slit or cut originally in the cake.

The heat in this trial was so moderate, that I was afraid that the crack had arisen from a want of tenacity, occasioned by the heat being too low.

The other half was heated a few shades higher, and subjected to the same mode of cutting; before the chissel had half way reached the bottom, the piece parted in two in the direction of the depression made by the cutting instrument. The additional heat in this instance proved an injury, while the cracking of the steel in both cases, particularly the former, was a certain proof of the abundance, or rather of the excess of the steely principle.

The fractures of both half cakes, now obtained for a second time, were materially different from that obtained by the simple division of the cake. The grain was nearly uniform, distinctly marked, but of too gray a colour for serviceable steel. Two of the quarters being drawn into neat bars under hand hammers at a low heat, one of them contained a number of cracks and fissures. The fracture was gray, tore out a little in breaking, but was otherwise yolky and excessively dense. A small bar of penknife size was improved greatly in drawing down, and had only one crack in thirteen inches of length. The grain and fracture were both highly improved by this additional labour; the tenacity of the steel was greater, and it stood firmly under the hammer at a bright red heat.

The other two quarters of this cake were squared a little, and successively put under a tilt hammer, of two hundred weight, going at the rate of three hundred blows per minute, and drawn into small penknife size. One of the bars from an outside piece, always the most solid, was entirely free from cracks, and had only one small scale running upon one side.

These bars exhibited a tougher break, than those drawn by hand; the colour was whiter, and the grain possessed a more regular and silky appearance.

Forging No. 2.

One half of this cake was heated to a scarlet shade, and put under the cutting chissel; it was at first struck lightly, then reheated, and cut comparatively soft; but a small crack had over-run the progress of the chissel. Its softness in cutting was attributed to an evident want of solidity. The other half cake felt harder under the hammer, but proved afterwards spongy throughout the mass. In the act of cutting, a loose pulverized matter was disengaged from some of the cells, possessed of a shining appearance.

The fractures obtained in consequence of the division of the half cakes, presented a flattish crystallized appearance, more resembling very white cast iron, than steel capable of being extended under the hammer. One of the middle cuts was entirely cellular with crystallized interiors, and incapable of drawing; the corresponding cut of the other half cake was drawn into a straight bar three quarters of an inch in breadth, and three-eighths thick, but was covered with cracks and flaws from end to end. The colour of the break was one shade lighter than No. 1, it tore less out, was equally yolky, and possessed on the whole an aspect very unfavourable for good

The other two outside quarters were also drawn into shape, one under the tilt hammer, and the other by hand. These were more solid in the fracture, possessed fewer surface-cracks, stood a higher degree of heat, tore out more, and exhibited a silky glossy grain, at least two shades lighter in the colour than the centre pieces.

Forging 3d Cake.

One half of this cake, first subjected to be cut, was found softer than any of the preceding, and exhibited no symptom of cracking. The other half was cut at three heats, but found loose and hollow in the extreme. A considerable portion of the same brilliant powder, formerly noticed, was here again disengaged. It was carefully taken up for examination, and found to be very fine ore of iron in a pulverescent state, very obedient to the magnet, and without any doubt an unmetallized portion of that from which wootz is made.

This curious circumstance led me to examine every pore and cell throughout the whole fragments. On the upper surface of two of them I found small pits containing a portion of the ore, which had been slightly agglutinated in the fire, but still highly magnetic. The upper surface of the present cake, close by the gate or feeder, contained a large pit filled with a stratum of semi-fused ore, surmounted by a mass of vitrified matter, which bore evident marks of containing calcareous earth.

Those who have devoted sufficient attention to the affinities of iron and earths for carbon, will be surprised to find that, on this particular subject, the rude fabricators of steel in Hindostan have got the start of our more polished countrymen in the manufacture of steel.

Two bars of wootz were formed from this cake, and these in point of quality inferior to any of those formerly produced. The appearance of the metal was more varied, less homogeneous, and contained more distinct laminæ with rusty surfaces, than either of the two former cakes.

It appeared highly probable, from the observations that occurred in forging, and in the examination of the cake, that the original proportion of mixture was such as would have formed a quality of steel softer than No. 1 and 2; but as steel of such softness requires a greater heat to fuse it, than when more fully saturated with carbonaceous matter, it is probable that the furnace had not been sufficiently powerful to occasion complete fusion of the whole mass, and generate a steel homogeneous in all its parts.

Forging 4th Cake.

Both halves of this cake cut pleasantly, and with a degree of tenacity and resistance, mixed at the same time with softness beyond what was experienced in any of the former cakes. Two quarters of this cake were drawn under the tilt hammer, and one by hand. The resulting bars were nearly perfect. A slight scale was observable upon the bar, from that quarter which contained the figure. The fracture was solid, though not homogeneous as to quality and colour, and it appeared pretty evident, that a considerable portion of one side through the whole bar was in the state of malleable iron, and of course not capable of being hardened. It was a subject of considerable regret, that the cake the most perfect and the most tenacious of the whole, in the process of forging, should get an imperfection which rendered it useless for the perfect purposes of steel.

Forging 5th Cake.

The first half of this cake cut uncommonly soft for wootz, but by cracking before the chissel still exhibited a want of proper tenacity. The next half cut equally soft, but with more tenacity. Two quarters of this cake drew readily out under the tilt hammer, and a third was drawn by hand at a bright red, sometimes approaching to a faint white heat. None of the bars thus obtained were uniformly free from cracks and scale, although the fracture exhibited a fair break of a light blue colour, and the grain was distinctly marked, and free from yolks.

General Remarks.

The formation of wootz appears to me to be in consequence of the fusion of a peculiar ore, perhaps calcareous, or rendered highly so by mixture of calcareous earth along with a portion of carbonaceous matter. That this is performed in a clay or other vessel or crucible, is equally presumable, in which the separated metal is allowed to cool; hence the crystallization that occupies the pits and cells found in and upon the under or rounded surface of the wootz cakes.

The want of homogeneity, and of real solidity in almost every cake of wootz, appears to me to be a direct consequence of the want of heat sufficiently powerful to effect a perfect reduction; what strengthens this supposition much is, that those cakes that are the hardest, *i. e.* that contain the greatest quantity of carbonaceous matter, and of course form the most fusible steel, are always the most solid and homogeneous. On the contrary, those cakes, into which the cutting chissel most easily finds its way, are in general cellular, replete with laminæ, and abound in veins of malleable iron.

It is probable, had the native Hindostan the means of rendering his cast steel as fluid as water, it would have occurred to him to have run it into moulds, and by this means have acquired an article uniform in its quality, and convenient for those purposes to which it is applied.

The hammering, which is evident around the feeder and upon the upper surface in general, may thus be accounted for. When the cake is taken from the pot or crucible, the feeder will most probably be slightly elevated, and the top of the cake partially covered with small masses of ore and steel iron, which the paucity of the heat had left either imperfectly separated or unfused. These most probably, to make the product more marketable, are cut off at a second heating, and the whole surface hammered smooth.

I have observed the same facts and similar appearances in operations of a like nature, and can account satisfactorily for it as follows.

The first portions of metal, that are separated in experiments of this nature, contain the largest share of the whole carbon introduced into the mixture. It follows of course, that an inferior degree of heat will maintain this portion of metal in a state of fluidity, but that a much higher temperature is requisite to reduce the particles of metal, thus for a season robbed of their carbon, and bring them into contact with the portion first rendered fluid, to receive their proportion of the steely principle. Where the heat is languid, the descent of the last portions of iron is sluggish, the mass below begins to lose its fluidity, while its disposition for giving out carbon is reduced by the gradual addition of more iron. An accumulation takes place of metallic masses of various diameters, rising up for half an inch or more into the glass that covers the metal; these are neatly welded and inserted into each other, and diminish in diameter as they go up. The length, or even the existence of this feeder or excrescence, depends upon the heat in general, and upon its temperature at different periods of the same process. If there has been sufficient heat, the surface will be convex and uniformly crystalline; but if the heat has been urged, after the feeder has been formed and an affinity established between it and the steelified mass below, it will only partially disappear in the latter, and the head or part of the upper end of the feeder will be found suspended in the glass that covers the steel.

The same or similar phenomena take place in separating crude iron from its ores, when highly carbonated, and difficult, from an excess of carbon, of being fused.

The division of the wootz cake by the manufacturers of Hindostan, I apprehend is merely to facilitate its subsequent application to the purposes of the artist; it may serve at the same time as a test of the quality of the steel.

To ascertain, by direct experiment, whether wootz owed its hardness to an extra quantity of carbon, the following experiments were performed with various portions of wootz of common cast steel, and of white crude iron, premising that in operations with iron and its ores, I have always found the comparative measure of carbon best ascertained by the quantity of lead which was reduced from flint glass.

1st Cake.

	Grains.
Fragments of wootz	65
Pounded flint glass three times the weight	195

This mixture was exposed to a heat of 160° WEDGEWOOD, and the wootz fused into a well crystallized spherule of steel. A thin crust of revived lead was found below the wootz, which weighed 9 grains, or $\frac{139}{1000}$ the weight of the wootz.

2d Cake.

Fragments of wootz	80
Flint glass, same proportion as above	240

The fusion of the mixture in this experiment was productive of a mass of lead weighing 10 grains, equal to $\frac{1}{8}$th the weight of the wootz.

3d Cake.

	Grains.
Fragments of wootz	75
Flint glass	225

The mass of lead precipitated beneath the steel in this experiment, amounted to 9 grains, or $\frac{12}{100}$ the weight of the wootz employed.

4th Cake.

Fragments of wootz	93
Flint glass	279

Lead obtained, precipitated from the glass by means of the carbon of the wootz 14½ grains, equal to $\frac{156}{1000}$ the weight of the wootz.

5th Cake.

Fragments of wootz	69
Flint glass	207

The lead revived in this experiment amounted to 7 grains, which is equal to $\frac{102}{1000}$ the weight of the wootz.

6*th. Cast Steel formed with* $\frac{1}{60}$*th part its Weight of Carbon.*

Fragments	90
Crystal glass	270

Lead revived 8½ grains equal to $\frac{94}{1000}$ the weight of the steel introduced.

7*th. White cast Iron dropt while Fluid into Water.*

Fragments	103
Crystal glass	309

The fusion of this precipitated 23½ grains of lead which is equal to $\frac{228}{1000}$ the weight of the cast iron.

Recapitulation of these Experiments.

1st cake of wootz revived of lead	,139
2d ditto	,125
3d ditto	,120
4th ditto	,156
5th ditto	,102
Steel containing $\frac{1}{60}$ of its weight of carbon	,094
Cast iron	,228

It would appear to result from these experiments, that wootz contains a greater proportion of carbonaceous matter, than the common qualities of cast steel in this country, and that some particular cakes approach considerably to the nature of cast iron. This circumstance, added to the imperfect fusion which generally occurs in the formation of wootz, appear to me to be quite sufficient to account for its refractory nature, and unhomogeneous texture.

Notwithstanding the many imperfections with which wootz is loaded, it certainly possesses the radical principles of good steel, and impresses us with a high opinion of the ore from which it is formed.

The possession of this ore for the fabrication of steel and bar iron, might to this country be an object of the highest importance. At present it is a subject of regret, that such a source of wealth cannot be annexed to its capital and talent. Were such an event practicable, then our East India Company might, in their own dominions, supply their stores with a valuable article, and at a much inferior price to any they send from this country.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1804.

1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Jan. 1	37	8	0	37	56	29,73	81	0,132	NNE	2	Cloudy.
	43	2	0	43	56	29,82	86		NNE	1	Cloudy.
2	36	8	0	39	53	29,82	91		NE	2	Cloudy.
	42	2	0	42	56	29,92	83		NE	2	Fine.
3	32	8	0	32	52	30,13	88		NE	1	Fair.
	40	2	0	40	53	30,15	83		NE	1	Fair.
4	30	8	0	31	50	30,16	83		E	1	Fair.
	35	2	0	35	52	30,12	83		ENE	1	Cloudy.
5	30	8	0	32	49	29,85	88		W	1	Snow.
	38	2	0	38	51	29,72	92		W	1	Cloudy.
6	28	8	0	28	49	29,76	85		WNW	1	Fine.
	34	2	0	34	52	29,79	83		NW	2	Fair.
7	27	8	0	28	48	29,82	89		W	1	Cloudy.
	35	2	0	35	52	29,72	85		WNW	1	Cloudy.
8	27	8	0	32	47	29,56	85		S	1	Cloudy.
	36	2	0	36	51	29,55	89		SSE	1	Cloudy.
9	28	8	0	39	48	29,68	91		E	1	Cloudy.
	42	2	0	42	50	29,73	91		SE	1	Cloudy.
10	40	8	0	38	49	29,92	84	0,235	SE	2	Cloudy. [Much wind last night.
	45	2	0	45	52	29,99	80		SE	1	Fair.
11	34	8	0	35	48	29,92	90		E	1	Cloudy.
	46	2	0	43	50	29,88	89		E	1	Cloudy.
12	43	8	0	45	50	29,64	96		SE	1	Cloudy.
	49	2	0	49	52	29,48	88		SSE	1	Cloudy.
13	47	8	0	52	52	29,32	96	0,023	S	2	Cloudy.
	52	2	0	52	54	29,22	95		S	2	Rain.
14	51	8	0	51	53	29,25	93	0,060	S	2	Cloudy.
	54	2	0	54	55	29,43	84		S	2	Cloudy.
15	48	8	0	54	53	29,56	96	0,197	S	2	Rain.
	55	2	0	55	56	29,54	97		S	2	Rain.
16	50	8	0	50	54	29,51	96	0,075	E	1	Cloudy.
	53	2	0	53	57	29,45	95		SSE	1	Rain.

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for January, 1804.

1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Jan. 17	49	8	0	49	55	29,41	95		E	1	Rain.
	54	2	0	54	57	29,37	94		SE	1	Rain.
18	50	8	0	50	57	29,58	92	0,025	S	2	Cloudy.
	51	2	0	51	58	29,55	92		S	2	Cloudy.
19	47	8	0	49	57	29,71	94		S	1	Cloudy.
	53	2	0	50	58	29,54	92		SE	1	Cloudy.
20	48	8	0	48	56	29,19	84	0,265	SSW	2	Cloudy. [Much wind last night.
	50	2	0	50	57	29,30	84		SSW	2	Cloudy.
21	45	8	0	50	56	29,43	94	0,055	S	2	Cloudy.
	54	2	0	54	58	29,49	94		S	2	Cloudy.
22	48	8	0	48	56	29,38	89		S	2	Fine.
	53	2	0	53	59	29,46	78		SW	2	Fair.
23	46	8	0	46	56	29,71	90	0,158	SW	1	Fair.
	52	2	0	51	58	29,78	84		SSW	2	Cloudy.
24	50	8	0	50	56	29,52	88		SSE	2	Cloudy. [Much wind last night.
	53	2	0	53	58	29,41	83		SSE	2	Fair.
25	48	8	0	48	56	29,37	90		SE	2	Cloudy.
	52	2	0	52	58	29,34	94		SE	2	Cloudy.
26	47	8	0	48	56	29,18	91		SE	2	Cloudy.
	52	2	0	52	58	29,17	83		S	2	Cloudy.
27	47	8	0	47	57	29,01	92		SSE	1	Rain.
	51	2	0	51	58	29,12	88		S	1	Cloudy.
28	47	8	0	51	56	28,88	92		S	2	Rain.
	53	2	0	51	57	28,68	93		S	2	Rain.
29	45	8	0	45	56	29,77	81	0,170	WNW	2	Fair.
	51	2	0	51	58	30,05	77		W	2	Fine.
30	43	8	0	47	57	30,18	91	0,068	SSE	1	Cloudy.
	51	2	0	51	57	30,06	90		S	2	Cloudy.
31	48	8	0	50	56	29,78	90	0,210	SSE	2	Cloudy.
	54	2	0	54	58	29,68	91		S	2	Cloudy.

METEOROLOGICAL JOURNAL

for February, 1804.

1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Feb. 1	45	7	0	45	57	29,62	90		SSW	1	Cloudy.
	51	2	0	51	59	29,55	82		SSW	2	Fair.
2	44	7	0	44	56	29,47	89	0,160	S	1	Rain.
	50	2	0	50	58	29,57	77		W	1	Fair.
3	45	7	0	45	56	29,38	92	0,310	SW	1	Rain.
	49	2	0	49	57	29,45	78		WSW	1	Cloudy.
4	34	7	0	34	54	29,41	83	0,022	WNW	1	Cloudy.
	38	2	0	37	55	29,61	87		NW	2	Fair.
5	31	7	0	32	52	30,05	83		WNW	2	Fair.
	43	2	0	43	55	29,93	77		WNW	2	Fair.
6	27	7	0	27	52	30,10	76		NW	2	Fine.
	32	2	0	32	54	30,13	71		NW	2	Fine.
7	25	7	0	26	49	30,34	87		NNE	2	Cloudy.
	35	2	0	35	52	30,46	82		NNE	1	Fair.
8	26	7	0	28	48	30,49	84		WNW	1	Cloudy.
	40	2	0	39	50	30,37	80		W	1	Cloudy.
9	39	7	0	43	49	30,05	93	0,090	SW	1	Cloudy.
	50	2	0	48	52	29,95	92		SW	1	Rain.
10	43	7	0	43	51	29,68	95	0,123	WSW	1	Fair.
	51	2	0	51	53	29,55	84		S	2	Cloudy.
11	43	7	0	43	51	29,05	91	0,310	SSW	2	Rain.
	50	2	0	48	53	29,05	92		W	1	Rain.
12	42	7	0	42	52	29,65	93	0,252	NE	1	Cloudy.
	44	2	0	39	52	30,02	78		NE	1	Snow.
13	31	7	0	31	50	30,26	79		NNE	2	Fine.
	36	2	0	36	53	30,32	74		NE	2	Fine.
14	27	7	0	28	48	30,38	78		NNE	2	Fine.
	36	2	0	36	50	30,37	69		NE	2	Fine.
15	27	7	0	28	48	30,39	85		NE	1	Fine.
	41	2	0	41	51	30,35	86		N	1	Cloudy.
16	35	7	0	36	49	30,32	90		NNE	1	Cloudy.
	41	2	0	41	51	30,30	85		NNE	1	Cloudy.

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for February, 1804.

1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Feb. 17	29	7	0	30	48	30,32	88		NNE	1	Fine.
	38	2	0	37	51	30,33	74		NNE	1	Fair.
18	34	7	0	38	48	30,20	89		N	2	Cloudy.
	42	2	0	42	52	30,31	77		NE	2	Fair.
19	33	7	0	35	49	30,33	91		NE	1	Cloudy.
	43	2	0	43	51	30,28	87		NNE	1	Cloudy.
20	38	7	0	38	50	30,34	92		NE	1	Cloudy.
	44	2	0	44	52	30,37	85		NE	1	Cloudy.
21	38	7	0	38	50	29,48	82		NNE	1	Cloudy.
	46	2	0	46	53	30,51	79		N	1	Cloudy.
22	42	7	0	42	52	30,46	82		N	1	Cloudy.
	46	2	0	46	53	30,41	78		NNW	1	Cloudy.
23	38	7	0	38	51	30,25	86		NNW	1	Fine.
	48	2	0	47	53	30,16	73		NW	1	Fair.
24	32	7	0	32	51	30,20	75		WNW	1	Cloudy.
	45	2	0	45	52	29,83	78		NW	2	Cloudy.
25	31	7	0	31	50	29,87	78	0,070	NW	2	Cloudy.
	38	2	0	38	52	29,84	72		NNW	1	Fair.
26	28	7	0	29	49	30,03	80		NW	1	Fine.
	41	2	0	41	52	30,05	76		NW	1	Cloudy.
27	39	7	0	40	50	29,91	91		W	1	Cloudy.
	47	2	0	47	52	29,92	82		NW	1	Cloudy.
28	37	7	0	37	50	29,91	87		W	1	Cloudy.
	44	2	0	38	52	29,93	80		NNE	1	Cloudy.
29	29	7	0	29	49	29,95	83		NNE	2	Fine.
	38	2	0	38	52	0,01	7		NE	2	Fair.

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for March, 1804.

1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Mar. 1	29	7	0	30	49	29,92	85		WNW	1	Cloudy.
	36	2	0	36	52	29,91	74		E	1	Fair.
2	28	7	0	28	48	29,91	85		NE	1	Fine.
	35	2	0	32	50	29,88	86		NE	2	Fair.
3	29	7	0	32	48	29,65	78		W	1	Cloudy.
	41	2	0	38	50	29,50	84		SE	1	Cloudy.
4	34	7	0	34	48	29,42	90	0,085	ESE	2	Cloudy.
	37	2	0	36	49	29,40	90		E	2	Snow.
5	36	7	0	39	48	29,35	88		E	1½	Cloudy.
	46	2	0	46	51	29,32	84		E	1	Cloudy.
6	39	7	0	39	48	29,30	92	0,128	ESE	1	Fair.
	46	2	0	46	51	29,33	82		SE	2	Cloudy.
7	37	7	0	38	49	29,58	91	0,185	SSE	1	Cloudy.
	50	2	0	50	52	29,68	81		SSE	1	Fair.
8	37	7	0	37	50	29,73	90		E	1	Fair.
	51	2	0	51	54	29,67	78		ESE	2	Fair.
9	45	7	0	46	52	29,53	88		E	1	Cloudy.
	52	2	0	51	53	29,47	91		ESE	1	Rain.
10	45	7	0	45	54	29,76	95		S	1	Cloudy.
	55	2	0	54	56	29,82	91		S	1	Fine.
11	45	7	0	46	54	29,78	96		E	1	Foggy.
	54	2	0	54	57	29,71	86		E	1	Fine.
12	43	7	0	43	55	29,68	90		E	1	Fine.
	57	2	0	57	58	29,78	82		SE	1	Fine.
13	46	7	0	47	56	29,82	90		E	1	Cloudy.
	59	2	0	59	58	29,85	73		SE	1	Fair.
14	44	7	0	44	56	29,86	83		E	1	Fine.
	62	2	0	62	59	29,85	68		S	1	Fine.
15	47	7	0	48	58	29,80	79		SE	1	Cloudy.
	62	2	0	62	60	29,75	68		S	1	Fair.
16	45	7	0	45	58	29,76	91		SE	1	Foggy.
	58	2	0	58	60	29,75	83		SE	1	Hazy.

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for March, 1804.

1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Mar. 17	46	7	0	46	58	29,74	91	0,028	E	1	Cloudy.
	58	2	0	58	60	29,68	85		E	1	Cloudy.
18	44	7	0	44	58	29,55	92	0,135	NE	1	Rain.
	44	2	0	41	59	29,63	93		NE	1	Rain.
19	36	7	0	36	57	29,58	92	0,238	N	1	Snow.
	40	2	0	39	56	29,53	85		NNE	1	Rain.
20	34	7	0	34	55	29,58	86	0,022	NE	1	Cloudy.
	35	2	0	34	55	29,66	83		NE	2	Cloudy.
21	32	7	0	32	53	29,87	82		NE	2	Cloudy.
	38	2	0	38	54	29,89	76		NE	2	Fine.
22	30	7	0	31	52	29,81	80		NE	2	Cloudy.
	39	2	0	39	54	29,74	72		ENE	2	Fine.
23	32	7	0	32	52	29,67	78		NE	2	Cloudy.
	42	2	0	42	52	29,66	73		NE	1	Fair.
24	33	7	0	34	51	29,72	85		NW	1	Fine.
	46	2	0	46	54	29,81	68		NW	1	Fine.
25	37	7	0	43	52	29,61	79		SW	2	Cloudy.
	49	2	0	48	54	29,43	78		SW	2	Cloudy.
26	42	7	0	44	51	29,27	91	0,065	S	2	Cloudy.
	50	2	0	50	54	29,15	81		SSE	2	Cloudy.
27	36	7	0	39	52	29,22	89	0,300	SSW	2	Cloudy.
	49	2	0	47	55	29,31	71		SSW	2	Cloudy.
28	38	7	0	38	52	29,54	91		NE	2	Cloudy.
	44	2	0	43	53	29,64	86		NE	1	Cloudy.
29	33	7	0	35	52	29,78	87		NE	1	Fair.
	51	2	0	51	55	29,76	77		W	1	Cloudy.
30	45	7	0	47	53	29,63	89		W	1	Cloudy.
	56	2	0	55	56	29,58	79		W	2	Cloudy.
31	43	7	0	44	53	29,21	90	0,353	W	1	Rain.
	49	2	0	49	56	29,18	78		WNW	1	Cloudy. This day there has been snow, hail, rain, and thunder and lightning.

METEOROLOGICAL JOURNAL

for April, 1804.

1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
April 1	36	7	0	36	53	29,33	80	0,345	WNW	2	Fair.
	45	2	0	44	55	29,39	70		NW	2	Fair.
2	35	7	0	37	52	29,29	81		WNW	1	Cloudy.
	47	2	0	47	55	29,22	69		NE	1	Hazy.
3	34	7	0	38	52	29,35	87		NE	1	Cloudy.
	48	2	0	47	54	29,51	71		NW	1	Cloudy.
4	36	7	0	39	52	29,67	85		W	1	Fine.
	52	2	0	50	55	29,59	79		SSW	2	Cloudy.
5	38	7	0	40	52	29,49	83		SSW	2	Cloudy.
	48	2	0	46	55	29,47	77		W	2	Cloudy.
6	37	7	0	39	52	30,00	80	0,100	WNW	1	Fine.
	52	2	0	51	55	30,14	70		NW	2	Fair.
7	41	7	0	41	53	30,25	78		NW	1	Cloudy.
	53	2	0	53	56	30,28	67		N	1	Cloudy.
8	41	7	0	42	53	30,27	84		E	1	Fine.
	53	2	0	52	56	30,24	65		E	1	Cloudy.
9	37	7	0	39	54	30,14	85		NE	1	Cloudy.
	49	2	0	49	56	30,03	76		NE	1	Cloudy.
10	41	7	0	41	53	29,89	90	0,022	NE	1	Rain.
	45	2	0	45	55	29,92	80		NE	1	Cloudy.
11	39	7	0	40	53	29,96	87		NE	1	Cloudy.
	43	2	0	43	55	29,95	82		NE	1	Cloudy.
12	39	7	0	41	53	29,87	90		NE	1	Cloudy.
	44	2	0	44	55	29,85	85		NE	1	Cloudy.
13	39	7	0	40	53	29,74	86		NE	1	Cloudy.
	45	2	0	45	55	29,72	80		ENE	1	Cloudy.
14	41	7	0	42	53	29,69	89		NE	1	Cloudy.
	54	2	0	54	56	29,67	74		ENE	1	Fair.
15	42	7	0	44	54	29,62	90		NE	1	Cloudy.
	58	2	0	58	57	29,58	80		NE	1	Cloudy.
16	46	7	0	46	55	29,58	86		NE	2	Cloudy.
	50	2	0	50	57	29,58	80		NE	2	Cloudy.

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1804	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Apr. 17	43	7	0	44	55	29,62	86		NE	2	Cloudy.
	47	2	0	46	57	29,62	86		NE	2	Cloudy.
18	41	7	0	42	55	29,70	88	0,075	NE	2	Cloudy.
	49	2	0	48	56	29,77	73		NE	2	Cloudy.
19	34	7	0	36	55	29,87	81		N	2	Fine.
	47	2	0	44	56	29,80	72		NW	2	Cloudy.
20	35	7	0	36	54	29,60	83	0,095	NW	2	Cloudy.
	45	2	0	43	56	29,67	82		NW	2	Cloudy.
21	36	7	0	37	54	29,64	77	0,055	NW	2	Fine.
	48	2	0	48	56	29,66	65		NW	2	Hazy.
22	34	7	0	37	54	29,65	86		WNW	1	Fine.
	51	2	0	50	56	29,66	65		WSW	1	Hazy.
23	35	7	0	42	53	29,55	90	0,155	SW	2	Cloudy.
	49	2	0	47	57	29,61	87		W	1	Rain.
24	44	7	0	50	55	29,62	94	0,390	S	2	Rain.
	58	2	0	58	57	29,67	81		S	2	Cloudy.
25	51	7	0	52	56	29,51	90	0,050	E	1	Cloudy.
	56	2	0	52	58	29,44	84		S	2	Rain.
26	47	7	0	49	56	29,45	87	0,062	SSW	2	Cloudy.
	58	2	0	58	59	29,55	75		W	2	Cloudy.
27	50	7	0	53	58	29,34	93	0,190	SSW	2	Cloudy.
	61	2	0	61	61	29,49	72		WSW	2	Fair.
28	50	7	0	51	59	29,61	87		SSE	1	Cloudy.
	60	2	0	60	60	29,66	86		S	1	Rain.
29	52	7	0	53	59	29,88	90	0,027	SSW	1	Cloudy.
	66	2	0	66	61	29,92	71		SSW	1	Fair.
30	52	7	0	53	60	29,90	88	0,038	SSE	1	Cloudy.
	71	2	0	70	61	29,86	75		SE	1	Fair.

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1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
May 1	56	7	0	58	62	29,80	87		ESE	1	Cloudy.
	71	2	0	70	63	29,78	76		SW	1	Hazy.
2	52	7	0	53	62	29,92	86		SW	1	Fine.
	68	2	0	68	64	29,93	71		E	1	Fair.
3	53	7	0	53	62	29,80	86		E	2	Cloudy.
	71	2	0	70	64	29,82	77		SSE	2	Fair.
4	58	7	0	60	63	29,86	86		NW	1	Cloudy.
	71	2	0	70	64	29,84	79		NNE	1	Cloudy.
5	56	7	0	58	64	29,84	92	0,016	NNE	1	Cloudy.
	73	2	0	73	66	29,98	70		ESE	1	Fair.
6	58	7	0	60	65	30,17	88		SSE	1	Cloudy.
	70	2	0	70	65	30,20	75		SW	1	Cloudy.
7	58	7	0	61	65	30,27	87		W	1	Cloudy.
	72	2	0	71	67	30,27	73		NW	1	Fair.
8	53	7	0	57	66	30,29	81		NE	1	Fair.
	65	2	0	65	67	30,22	68		NE	1	Fair.
9	50	7	0	53	64	30,12	81		NE	1	Cloudy.
	67	2	0	67	65	29,99	69		SSW	1	Fair.
10	53	7	0	53	63	29,91	87	0,178	WNW	1	Fair.
	63	2	0	62	64	29,93	67		NW	1	Cloudy.
11	49	7	0	50	63	29,93	80		NW	1	Fine.
	62	2	0	59	64	29,94	67		NW	1	Cloudy.
12	46	7	0	42	61	30,05	72	0,075	NNW	1	Fair.
	58	2	0	57	62	30,06	66		NNW	1	Fair.
13	44	7	0	47	61	30,08	78		W	1	Fine.
	64	2	0	62	63	30,08	64		NE	1	Fair.
14	47	7	0	52	60	30,05	74		E	1	Hazy.
	65	2	0	64	62	30,07	70		SSW	1	Fair.
15	50	7	0	53	61	30,06	75		SSE	1	Cloudy.
	70	2	0	70	62	30,02	68		S	1	Fair.
16	56	7	0	60	62	29,88	77		SSE	1	Cloudy.
	69	2	0	68	62	29,78	72		SE	1	Cloudy.

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1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
May 17	53	7	0	53	62	29,67	88	0,373	SW	1	Fair.
	67	2	0	67	63	29,78	67		W	1	Fair.
18	51	7	0	54	62	30,03	86		SSW	1	Cloudy.
	66	2	0	66	63	30,01	70		S	1	Fair.
19	54	7	0	56	62	29,86	81	0,033	SE	1	Cloudy.
	68	2	0	68	63	29,77	69		SE	1	Hazy.
20	52	7	0	57	62	29,74	77		NNE	1	Fair.
	70	2	0	69	64	29,77	67		NE	2	Fair.
21	51	7	0	54	62	29,83	73		ENE	2	Fair.
	66	2	0	65	64	29,73	71		E	2	Fair.
22	56	7	0	58	63	29,78	90	0,058	SE	2	Rain.
	65	2	0	63	63	29,85	73		W	2	Cloudy.
23	52	7	0	53	62	30,07	86		SSW	1	Cloudy.
	67	2	0	67	63	30,07	69		S	1	Fair.
24	55	7	0	56	62	29,80	88	0,047	S	2	Rain.
	61	2	0	61	62	29,64	87		S	2	Rain.
25	51	7	0	52	61	29,68	82	0,090	SW	2	Fair.
	65	2	0	65	62	29,75	70		SW	2	Fair.
26	54	7	0	54	62	29,67	87		S	2	Fine.
	67	2	0	67	62	29,69	67		S	2	Hazy.
27	51	7	0	53	61	29,86	82		S	2	Fair.
	66	2	0	65	62	29,90	68		S	2	Fair.
28	51	7	0	56	61	29,86	80		S	2	Fair.
	65	2	0	63	62	29,78	69		SSE	2	Cloudy.
29	54	7	0	55	61	29,71	87	0,333	W	2	Cloudy.
	66	2	0	66	62	29,91	70		WNW	2	Fair.
30	48	7	0	51	60	30,10	84	0,045	W	2	Fine.
	64	2	0	64	61	30,06	72		S	2	Fair.
31	53	7	0	53	61	29,96	81		WNW	1	Fine.
	68	2	0	68	62	29,99	68		WNW	2	Fair.

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1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
June 1	51	7	0	55	61	30,14	82		WSW	1	Fair.
	70	2	0	70	63	30,14	67		WSW	1	Fair.
2	53	7	0	55	61	30,12	83		WSW	2	Cloudy.
	75	2	0	74	63	30,06	62		SSW	1	Fine.
3	56	7	0	58	63	30,02	81		SW	1	Fine.
	79	2	0	78	66	30,01	62		S	1	Fine.
4	60	7	0	66	65	29,97	76		E	1	Cloudy.
	81	2	0	80	71	29,94	72		S	1	Fine.
5	60	7	0	61	67	30,03	81		W	1	Cloudy.
	75	2	0	75	68	30,04	72		WSW	1	Cloudy.
6	61	7	0	63	67	30,02	82		WSW	1	Cloudy.
	71	2	0	70	68	29,94	73		S	2	Cloudy.
7	54	7	0	56	66	29,91	80		S	2	Cloudy.
	69	2	0	69	68	29,79	68		SSW	2	Cloudy.
8	51	7	0	55	65	29,69	78		SW	2	Cloudy.
	66	2	0	64	66	29,68	70		SW	2	Cloudy.
9	53	7	0	53	64	29,94	77		W	2	Fair.
	67	2	0	67	66	30,04	66		WNW	2	Fair.
10	50	7	0	53	64	30,12	82		SW	1	Cloudy.
	63	2	0	63	64	30,10	82		WSW	1	Cloudy.
11	47	7	0	50	63	30,36	79	0,260	NE	1	Fine.
	66	2	0	65	64	30,38	68		NE	1	Fair.
12	49	7	0	53	63	30,40	83		NE	1	Fair.
	70	2	0	70	65	30,40	70		ENE	1	Fine.
13	57	7	0	58	63	30,36	76		ESE	1	Cloudy.
	70	2	0	70	64	30,31	73		ESE	1	Cloudy.
14	61	7	0	62	64	30,20	83		E	1	Cloudy.
	69	2	0	64	64	30,13	83		N	1	Rain.
15	57	7	0	57	64	30,00	87	0,262	NE	1	Fine.
	70	2	0	69	65	29,94	72		NE	1	Cloudy.
16	52	7	0	54	64	29,83	84		E	1	Cloudy.
	70	2	0	69	66	29,76	78		E	1	Fair.

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1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
June 17	56	7	0	58	64	29,83	87		SW	1	Fine.
	72	2	0	71	66	29,92	69		WNW	1	Cloudy.
18	57	7	0	58	65	30,10	83		W	1	Fine.
	73	2	0	72	66	30,19	70		W	1	Fine.
19	55	7	0	57	63	30,37	80		SW	1	Hazy.
	68	2	0	68	65	30,36	73		SSW	1	Cloudy.
20	60	7	0	62	65	30,34	83		SW	2	Cloudy.
	76	2	0	75	68	30,33	72		WNW	1	Fair.
21	58	7	0	60	66	30,36	88		WSW	1	Cloudy.
	81½	2	0	81	68	30,31	71		SSW	1	Fine.
22	61	7	0	64	68	30,33	81		WNW	1	Fine.
	78	2	0	77	70	30,30	70		NE	1	Hazy.
23	56	7	0	58	68	30,34	77		E	1	Fine.
	68	2	0	68	68	30,30	70		E	1	Fine.
24	56	7	0	60	68	30,25	83		E	1	Fine.
	75	2	0	75	69	30,21	66		E	1	Fine.
25	59	7	0	63	68	30,18	79		ENE	1	Fine.
	87	2	0	85	71	30,17	63		SE	1	Fair.
26	57	7	0	60	68	30,24	83		NE	1	Cloudy.
	68	2	0	68	69	30,20	75		E	1	Cloudy.
27	56	7	0	58	68	30,04	77		NE	1	Fair.
	68	2	0	67	68	30,00	70		NE	1	Fair.
28	49	7	0	53	67	30,11	78		NE	2	Fair.
	63	2	0	61	67	30,16	71		NE	2	Cloudy.
29	48	7	0	52	65	30,14	78		W	1	Fair.
	72	2	0	72	67	30,02	64		WSW	1	Fine.
30	54	7	0	56	66	29,97	77		WSW	1	Fair.
	74	2	0	74	67	29,90	66		SW	1	Fine.

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1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
July 1	58	7	0	60	66	29,81	73		SSE	1	Fine.
	73	2	0	73	68	29,82	67		ESE	1	Fine.
2	56	7	0	61	67	29,86	80		E	1	Fair.
	72	2	0	72	68	29,80	67		E	1	Fair.
3	57	7	0	62	67	29,70	78		ENE	1	Cloudy.
	65	2	0	65	68	29,68	81		NW	1	Cloudy.
4	54	7	0	55	65	29,82	82	0,032	NW	2	Fine.
	71	2	0	71	67	29,85	63		NW	1	Fine.
5	55	7	0	56	63	29,82	79		SW	1	Rain.
	65	2	0	65	65	29,75	72		SSE	1	Cloudy.
6	54	7	0	55	65	29,71	90	0,200	NE	1	Rain.
	63	2	0	63	65	29,83	85		ESE	1	Cloudy.
7	57	7	0	60	65	29,83	90	0,075	SE	1	Cloudy.
	63	2	0	62	65	29,75	90		S	1	Rain.
8	56	7	0	58	64	29,68	90	0,415	SW	1	Fair.
	71	2	0	71	66	29,68	71		SW	1	Fair.
9	55	7	0	57	65	29,78	87	0,105	WSW	1	Cloudy.
	73	2	0	73	66	29,77	71		SW	1	Fair.
10	58	7	0	58	65	29,74	86	0,035	E	1	Rain.
	62	2	0	62	65	29,64	94		E	1	Rain.
11	51	7	0	53	64	29,98	81	2,090	NNE	2	Fine.
	60	2	0	60	64	30,11	67		NE	1	Cloudy.
12	50	7	0	53	63	30,23	71		N	1	Cloudy.
	64	2	0	63	64	30,21	64		N	1	Cloudy.
13	49	7	0	53	63	30,21	77		NE	1	Fair.
	68	2	0	68	65	30,17	63		NE	1	Fair.
14	53	7	0	57	62	30,16	78		E	1	Cloudy.
	66	2	0	66	64	30,10	69		E	1	Cloudy.
15	51	7	0	53	63	30,08	82		E	1	Fine.
	73	2	0	71	65	30,04	65		ENE	1	Fine.
16	58	7	0	60	64	30,11	80		NE	1	Fine.
	78	2	0	77	67	30.13	62		NE	1	Fine.

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	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
July 17	61	7	0	64	66	30,12	78		WSW	1	Fair.
	80	2	0	79	68	30,05	63		S	2	Fine.
18	63	7	0	68	68	29,83	73		SW	1	Cloudy.
	75	2	0	74	69	29,76	68		SSW	1	Cloudy.
19	61	7	0	62	67	29,57	83	0,062	SSW	1	Rain.
	74	2	0	72	69	29,55	68		WSW	1	Fair.
20	58	7	0	61	67	29,65	77	0,190	NE	1	Fair.
	67	2	0	63	67	29,72	83		NE	1	Cloudy.
21	58	7	0	58	66	29,79	82		NNE	2	Cloudy.
	67	2	0	67	68	29,85	70		NE	2	Fair.
22	55	7	0	58	66	29,73	78		E	1	Cloudy.
	69	2	0	67	67	29,53	78		SSW	1	Cloudy.
23	55	7	0	57	66	29,44	81	0,053	SSE	2	Fair.
	68	2	0	67	66	29,44	73		SSE	2	Fair.
24	57	7	0	60	66	29,62	80		ENE	1	Cloudy.
	73	2	0	70	67	29,64	66		NW	1	Cloudy.
25	56	7	0	57	64	29,61	75		WNW	1	Cloudy.
	71	2	0	71	67	29,54	63		WNW	1	Fair.
26	58	7	0	60	66	29,44	90	0,390	NE	1	Rain.
	70	2	0	70	67	29,51	66		NE	1	Fair.
27	55	7	0	57	66	29,54	81		SW	1	Fine.
	66	2	0	66	66	29,48	73		S	2	Cloudy.
28	54	7	0	57	65	29,50	81	0,042	S	2	Fair.
	68	2	0	67	66	29,55	73		S	2	Fair.
29	57	7	0	58	65	29,65	79		S	2	Fair.
	72	2	0	72	67	29,76	69		S	2	Fair.
30	55	7	0	56	65	30,03	83	0,016	S	1	Cloudy.
	76	2	0	75	68	30,05	66		WSW	1	Fine.
31	58	7	0	59	65	30,20	75		W	1	Fine.
	78	2	0	77	69	30,20	60		W	1	Fine.

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1804	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Aug. 1	° 68	7	0	70	69	30,27	° 78		NE	1	Cloudy.
	79	2	0	79	70	30,27	67		NE	1	Cloudy.
2	61	7	0	62	68	30,16	78		NE	1	Cloudy.
	71	2	0	71	69	30,06	75		NE	1	Cloudy.
3	61	7	0	63	68	29,92	91		NE	1	Cloudy.
	81	2	0	81	70	29,83	74		ENE	1	Fair.
4	64	7	0	65	69	29,78	87		NNW	1	Cloudy.
	74	2	0	72	70	29,76	78		NW	1	Cloudy.
5	57	7	0	61	69	29,84	84	0,090	W	1	Cloudy.
	74	2	0	73	70	29,81	67		SSW	2	Fair.
6	58	7	0	59	66	29,91	80		WSW	2	Fair.
	73	2	0	72	69	29,98	62		WNW	1	Fair.
7	53	7	0	54	67	30,10	78		W	1	Fair.
	71	2	0	71	68	30,11	63		NW	2	Fair.
8	58	7	0	58	65	29,68	86	0,300	S	2	Rain.
	73	2	0	73	68	29,55	67		W	2	Fair.
9	54	7	0	55	67	29,86	79		WSW	1	Fair.
	73	2	0	72	68	29,81	65		S	2	Cloudy.
10	58	7	0	58	65	29,56	80		S	2	Fair.
	70	2	0	69	67	29,66	71		W	2	Cloudy.
11	55	7	0	56	66	30,00	78		SSW	2	Fine.
	70	2	0	70	66	29,01	65		SW	2	Cloudy.
12	57	7	0	57	65	29,70	80	0,016	S	2	Cloudy.
	65	2	0	63	66	29,62	71		S	2	Cloudy.
13	53	7	0	56	65	29,61	80	0,072	S	2	Cloudy.
	63	2	0	57	64	29,18	93		E	1	Rain.
14	49	7	0	50	63	29,47	85	0,880	W	1	Fine.
	70	2	0	70	65	29,53	65		S	1	Fair.
15	54	7	0	55	63	29,58	88		S	1	Cloudy.
	71	2	0	71	65	29,57	77		SSW	2	Cloudy.
16	57	7	0	57	64	29,57	91	1,030	SSW	1	Rain.
	69	2	0	68	66	29,64	72		WSW	2	Fair.

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	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Aug. 17	54	7	0	55	64	29,80	83	0,075	WSW	2	Cloudy.
	68	2	0	64	65	29,87	73		WSW	2	Fair.
18	50	7	0	52	63	30,00	80	0,135	SW	1	Rain.
	63	2	0	62	64	29,92	73		W	1	Cloudy.
19	54	7	0	55	63	29,77	84	0,110	NE	1	Cloudy.
	67	2	0	65	64	29,80	69		N	1	Cloudy.
20	51	7	0	52	62	29,87	82		W	1	Cloudy.
	64	2	0	64	63	29,88	71		NW	1	Cloudy.
21	53	7	0	54	62	29,92	83		W	1	Cloudy.
	63	2	0	62	63	29,94	72		NW	1	Cloudy.
22	56	7	0	56	62	29,97	77		NE	1	Cloudy.
	66	2	0	66	64	30,00	66		NE	1	Fair.
23	54	7	0	54	62	30,11	78		N	1	Cloudy.
	63	2	0	63	63	30,13	70		NE	1	Cloudy.
24	54	7	0	55	62	30,17	78		W	1	Cloudy.
	66	2	0	66	63	30,17	67		WNW	1	Cloudy.
25	53	7	0	55	62	30,14	80		N	1	Cloudy.
	64	2	0	64	63	30,17	68		NNE	1	Cloudy.
26	56	7	0	56	62	30,28	77		NE	1	Cloudy.
	67	2	0	66	63	30,30	68		NE	1	Fair.
27	52	7	0	52	62	30,24	78		SW	1	Fine.
	71	2	0	70	64	30,16	65		W	1	Fair.
28	59	7	0	60	63	30,17	83		WSW	1	Cloudy.
	72	2	0	72	65	30,19	72		WSW	1	Fair.
29	58	7	0	58	65	30,17	85		SW	1	Fine.
	77	2	0	75	67	30,12	71		S	1	Fine.
30	63	7	0	64	67	29,90	88	0,093	NE	1	Fair.
	80	2	0	80	69	29,89	75		S	1	Fine.
31	63	7	0	63	68	30,07	80		NE	1	Fair.
	73	2	0	72	69	30,09	73		NE	1	Cloudy.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Sept. 1	° 58	7	0	58	66	30,00	° 76		W	1	Fine.
	68	2	0	68	68	30,10	63		WNW	1	Fine.
2	54	7	0	55	67	30,20	80		SW	1	Fine.
	69	2	0	69	68	30,18	67		SW	1	Fair.
3	53	7	0	55	65	30,20	81		SW	1	Fine.
	68	2	0	67	67	30,21	75		SSW	2	Cloudy.
4	58	7	0	58	66	30,28	88		SW	1	Cloudy.
	73	2	0	73	67	30,32	73		SSW	1	Cloudy.
5	58	7	0	58	67	30,30	87		WNW	1	Fair.
	73	2	0	73	68	30,27	68		S	1	Cloudy.
6	57	7	0	57	65	30,13	85		NE	1	Fair.
	72	2	0	72	70	30,02	68		ENE	1	Fine.
7	55	7	0	55	67	29,94	84		E	1	Fine.
	72	2	0	72	69	29,97	69		SW	1	Fair.
8	53	7	0	55	67	30,16	82		W	1	Fair.
	70	2	0	70	69	30,20	68		E	1	Fine.
9	54	7	0	55	68	30,22	80		ENE	1	Fine.
	70	2	0	70	69	30,15	70		E	1	Fine.
10	52	7	0	55	66	30,00	82		NE	1	Fine.
	74	2	0	74	68	29,98	73		S	2	Cloudy.
11	57	7	0	57	67	30,14	84		SW	1	Fair.
	69	2	0	68	69	30,17	66		SSW	1	Fair.
12	55	7	0	57	68	30,18	78		SW	1	Fine.
	79	2	0	78	72	30,12	73		SSE	1	Fine.
13	62	7	0	64	70	30,00	83		S	1	Fine.
	81	2	0	81	73	30,00	70		S	2	Fine.
14	62	7	0	62	68	30,02	86		S	1	Cloudy.
	79	2	0	79	73	29,92	73		SE	1	Fine.
15	63	7	0	64	70	29,96	83		S	1	Cloudy.
	78	2	0	78	73	29,96	73		SE	2	Fine.
16	62	7	0	63	70	30,01	91		NE	1	Cloudy.
	81½	2	0	81	74	30,07	70		E	1	Fine.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Sep. 17	63°	7	0	64	72	30,18	88°		NE	1	Cloudy.
	67	2	0	66	70	30,18	83		NE	2	Cloudy.
18	59	7	0	60	70	30,18	81		NE	2	Cloudy.
	63	2	0	62	69	30,20	78		NE	1	Cloudy.
19	59	7	0	59	69	30,11	80		ENE	1	Cloudy.
	68	2	0	68	68	30,07	74		S	1	Fair.
20	61	7	0	62	68	30,10	84		WSW	1	Cloudy.
	71	2	0	70	70	30,08	66		NW	1	Fine.
21	52	7	0	55	68	30,10	79		WNW	1	Cloudy.
	66	2	0	65	68	30,05	66		W	1	Fair.
22	50	7	0	52	66	30,14	76		WSW	1	Cloudy.
	63	2	0	63	65	29,98	69		WSW	1	Cloudy.
23	49	7	0	50	65	29,88	76		WNW	1	Cloudy.
	59	2	0	58	64	29,96	65		NNW	2	Fair.
24	48	7	0	50	63	30,02	78		NNW	1	Cloudy.
	59	2	0	57	63	30,05	70		N	2	Cloudy.
25	49	7	0	50	62	30,18	84		NE	2	Cloudy.
	62	2	0	61	63	30,29	70		NE	2	Fair.
26	45	7	0	47	61	30,46	80		NE	2	Fine.
	59	2	0	58	63	30,47	70		NE	1	Fine.
27	49	7	0	56	61	30,37	84		NE	1	Cloudy.
	62	2	0	62	62	30,34	77		NE	1	Cloudy.
28	53	7	0	53	61	30,34	88		NE	1	Cloudy.
	60	2	0	60	62	30,32	68		NE	1	Cloudy.
29	52	7	0	52	61	30,27	88		NE	1	Cloudy.
	59	2	0	59	61	30,18	83		E	1	Cloudy.
30	48	7	0	50	60	30,00	87		E	1	Foggy.
	61	2	0	60	61	29,92	84		E	1	Rain.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Oct. 1	57	7	0	57	60	29,77	90	0,077	ESE	1	Rain.
	64	2	0	64	61	29,76	87		NE	1	Cloudy.
2	53	7	0	53	60	29,89	92	0,045	SW	1	Cloudy.
	64	2	0	64	62	29,96	86		SW	1	Cloudy.
3	57	7	0	59	61	29,96	94		S	2	Cloudy.
	68	2	0	68	63	29,89	74		S	2	Fair.
4	52	7	0	52	62	29,97	90		S	1	Foggy.
	63	2	0	62	63	29,92	77		S	2	Fine.
5	57	7	0	57	62	29,57	92	0,142	SW	1	Cloudy.
	63	2	0	62	63	29,72	69		W	1	Fair.
6	45	7	0	45	61	30,14	83		SW	1	Fine.
	63	2	0	63	63	30,16	70		SW	2	Fair.
7	56	7	0	57	62	30,03	90		SW	1	Cloudy.
	61	2	0	61	62	29,90	80		S	2	Cloudy.
8	46	7	0	47	60	29,77	85	0,045	SW	1	Fine.
	59	2	0	58	61	29,76	70		W	1	Fair.
9	44	7	0	44	59	29,84	83		WNW	1	Fine.
	56	2	0	56	61	29,95	68		NW	2	Fair.
10	38	7	0	40	58	30,13	82		W	1	Fair.
	58	2	0	56	60	30,02	77		S	1	Rain.
11	53	7	0	58	59	29,52	95	0,345	SSW	1	Rain.
	61	2	0	61	61	29,38	95		S	1	Cloudy.
12	44	7	0	46	59	29,37	86	0,350	SW	1	Fair.
	55	2	0	54	60	29,29	77		SW	1	Rain.
13	38	7	0	38	58	29,46	85	0,037	W	1	Cloudy.
	52	2	0	52	60	29,52	78		W	1	Fair.
14	44	7	0	52	58	29,27	93	0,023	S	2	Rain.
	58	2	0	58	60	29,27	73		SW	1	Cloudy.
15	48	7	0	48	58	29,25	86	0,048	SW	1	Fair.
	55	2	0	55	61	29,33	68		WSW	1	Fine.
16	39	7	0	39	57	29,61	84		WSW	1	Fine.
	54	2	0	54	61	29,73	70		WSW	1	Fine.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Oct. 17	46	7	0	48	58	29,67	88	0,030	SSW	2	Fair. [Lightning with thunder.
	60	2	0	59	60	29,66	72		SSW	2	Cloudy.
18	46	7	0	48	58	29,97	84	0,063	SW	2	Cloudy.
	60	2	0	60	59	29,96	78		WNW	2	Cloudy.
19	53	7	0	53	59	30,01	90		SSW	2	Cloudy.
	61	2	0	60	62	29,96	76		S	2	Fair.
20	53	7	0	56	59	29,85	86		S	2	Fair.
	60	2	0	64	62	29,88	82		S	2	Fair.
21	54	7	0	54	60	29,77	91	0,072	E	1	Cloudy.
	63	2	0	62	62	29,66	81		E	1	Cloudy.
22	56	7	0	56	61	29.43	90		E	1	Cloudy.
	59	2	0	57	62	29,41	87		S	1	Rain. The Aurora Borealis was visible from 6 to 9 P. M; at times, it was very brilliant, but had not much motion.
23	41	7	0	41	60	29,57	84	0,290	SW	1	Fair.
	56	2	0	56	62	29,61	77		NE	1	Fair.
24	44	7	0	44	60	29,77	83		NNE	1	Fine.
	53	2	0	52	62	29,78	83		NE	1	Fine.
25	45	7	0	46	60	29,80	83		NE	1	Cloudy.
	53	2	0	53	60	29,78	82		NE	1	Cloudy.
26	49	7	0	50	59	29,76	89		E	1	Fine.
	60	2	0	58	61	29,78	81		SE	1	Cloudy.
27	50	7	0	50	59	29,76	93	0,330	E	1	Rain.
	54	2	0	53	60	29.74	87		SE	1	Rain.
28	43	7	0	44	58	29,68	90	0,022	E	1	Foggy.
	55	2	0	55	61	29,62	80		ESE	1	Fair.
29	49	7	0	49	59	29,55	90		E	1	Cloudy.
	52	2	0	52	60	29,52	90		E	1	Rain.
30	50	7	0	50	59	29.30	90	0,042	ENE	1	Fair.
	55	2	0	55	60	29,18	80		E	2	Cloudy.
31	50	7	0	50	59	29,38	93	0,085	S	1	Fair.
	60	2	0	60	61	29,55	81		S	1	Fair.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Nov. 1	48	7	0	49	59	29,55	90	0,515	S	1	Fine.
	59	2	0	58	61	29,60	82		SW	1	Cloudy.
2	51	7	0	51	60	29,73	90		S	1	Rain.
	56	2	0	56	62	29,83	88		NE	1	Cloudy.
3	47	7	0	47	59	30,23	85	0,133	NE	2	Cloudy.
	49	2	0	48	61	30,27	73		NE	2	Fine.
4	42	7	0	42	58	30,15	77		NE	2	Cloudy.
	45	2	0	44	59	30,03	77		NE	2	Fair.
5	40	7	0	40	57	29,90	80		NE	2	Cloudy. [Much wind last night.
	45	2	0	45	57	29,90	77		NE	2	Cloudy.
6	38	7	0	38	55	29,98	77		ENE	1	Cloudy.
	42	2	0	42	56	29,98	74		ENE	1	Cloudy.
7	38	7	0	39	54	29,90	77		ESE	1	Cloudy.
	46	2	0	46	55	29,81	78		SE	1	Cloudy.
8	45	7	0	49	55	29,53	92	0,063	SE	1	Rain.
	54	2	0	52	57	29,41	88		ESE	1	Cloudy.
9	42	7	0	42	56	29,38	92		ESE		Foggy.
	52	2	0	52	57	29,43	91		ESE	1	Foggy.
10	49	7	0	50	57	29,24	93	0,160	ESE	1	Rain.
	58	2	0	57	59	29,22	91		SW	1	Rain.
11	52	7	0	52	58	29,25	94	0,560	NE	1	Rain.
	52	2	0	52	59	29,37	94		NE	1	Rain.
12	47	7	0	47	57	29,94	92	0,310	ENE	1	Rain.
	56	2	0	56	59	29,96	89		S	1	Cloudy.
13	51	7	0	52	58	29,70	92	0,200	SSE	2	Rain.
	59	2	0	59	60	29,66	93		SSW	2	Cloudy.
14	51	7	0	53	58	29,57	94	0,093	S	2	Rain.
	54	2	0	54	60	29,55	94		S	1	Rain.
15	45	7	0	45	58	29,78	91	0,375	NE	1	Cloudy.
	46	2	0	46	59	29,84	89		NE	1	Cloudy.
16	44	7	0	44	58	29,98	92		NE	1	Cloudy.
	48	2	0	48	58	30,00	92		NE	1	Rain.

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	°	H.	Mi	°	°	Inches.	°	Inches.	Points.	Str.	
Nov. 17	45	7	0	46	57	30,09	92		E	1	Cloudy.
	50	2	0	49	59	30,12	86		E	1	Fair.
18	45	7	0	46	57	30,18	90		E	1	Cloudy.
	52	2	0	52	59	30,16	88		SE	1	Cloudy.
19	48	7	0	51	57	30,10	93		S	2	Cloudy.
	53	2	0	53	60	29,94	93		S	1	Rain.
20	44	7	0	44	57	30,13	92	0,564	SW	1	Cloudy.
	55	2	0	50	60	30,05	86		S	1	Fair.
21	48	7	0	53	58	29.50	88	0,275	WSW	2	Cloudy.
	53	2	0	51	60	29,76	73		W	2	Fair.
22	44	7	0	45	57	29.76	86		W	1	Fine.
	48	2	0	48	58	29,89	73		NW	1	Fair.
23	38	7	0	42	57	29,97	85		WSW	1	Cloudy.
	50	2	0	50	58	29,84	85		SW	1	Rain.
24	39	7	0	39	56	29,85	87	0,235	ENE	1	Rain.
	39	2	0	37	57	29,80	93		NE	1	Rain.
25	36	7	0	43	55	29,84	81	0,510	E	2	Rain.
	44	2	0	44	56	29,89	76		E	2	Cloudy.
26	35	7	0	37	53	30,04	80		NE	2	Cloudy.
	44	2	0	44	56	30,08	80		NE	1	Fair.
27	35	7	0	36	52	30,00	91		NE	1	Cloudy.
	41	2	0	41	55	30,05	77		NE	1	Fair.
28	37	7	0	37	52	30,02	77		ENE	2	Cloudy.
	36	2	0	36	53	29,98	79		ENE	2	Cloudy.
29	34	7	0	35	51	29 98	88		NE	1	Cloudy.
	38	2	0	38	53	29,97	83		NE	1	Cloudy.
30	34	7	0	34	50	29,95	82		NE	1	Fair.
	40	2	0	40	51	29,96	81		NE	1	Cloudy.

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	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Dec. 1	35	8	0	35	50	29,96	90		NE	1	Cloudy.
	42	2	0	42	53	30,05	83		NE	1	Cloudy.
2	35	8	0	35	50	30,28	90		NE	1	Cloudy.
	39	2	0	38	53	30,30	88		NE	1	Cloudy.
3	36	8	0	37	51	30,33	86		E	1	Cloudy.
	38	2	0	37	53	30,31	80		E	1	Fine.
4	28	8	0	29	49	29,98	84		E	1	Fine.
	39	2	0	39	51	29,78	83		E	1	Cloudy.
5	39	8	0	45	51	29,33	93	0,035	E	1	Foggy.
	48	2	0	48	53	29,08	94		E	1	Cloudy.
6	37	8	0	37	51	29,05	95		S	1	Foggy.
	41	2	0	41	53	29,11	94		S	1	Foggy.
7	38	8	0	43	52	29,43	94		NNE	1	Cloudy.
	45	2	0	45	54	29,54	91		NNE	1	Cloudy.
8	43	8	0	43	52	29,83	93		NE	1	Cloudy.
	45	2	0	45	54	29,90	90		ENE	1	Cloudy.
9	40	8	0	40	52	29,88	90		E	1	Fair.
	45	2	0	45	53	29,86	90		SE	1	Rain.
10	40	8	0	40	53	29,94	94	0,093			Foggy.
	47	2	0	47	54	29,92	94		S	1	Cloudy.
11	44	8	0	44	53	29.78	94		S	1	Fair.
	50	2	0	50	55	29,71	93		S	1	Cloudy.
12	41	8	0	41	53	29,52	92	0,112	SSW	2	Fine.
	51	2	0	47	55	29,50	88		S	1	Cloudy.
13	46	8	0	46	53	29,06	90	0,030	S	2	Fair. { Much wind last night.
	48	2	0	48	56	29,07	83		S	2	Fine.
14	44	8	0	44	54	29,31	83	0,030	WSW	1	Fair.
	48	2	0	48	56	29,49	80		WNW	1	Fair.
15	42	8	0	43	54	29,78	83		WNW	2	Cloudy.
	47	2	0	47	58	29,92	81		NW	2	Fine.
16	35	8	0	35	53	30,10	87		N	1	Fine.
	41	2	0	41	56	30,10	83		NNW	1	Cloudy.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Dec. 17	32	8	0	32	52	29,95	83		NE	1	Fair.
	35	2	0	35	55	29,96	83		NE	2	Fair.
18	31	8	0	32	51	30,15	87		NE	2	Cloudy.
	37	2	0	37	54	30,21	84		NE	2	Fair.
19	29	8	0	29	49	30,39	87		ENE	2	Snow.
	31	2	0	30	51	30,40	84		NE	2	Snow.
20	28	8	0	28	48	30,25	82		ENE	2	Cloudy. [Much wind last night.
	32	2	0	30	49	30,18	79		ENE	2	Fair.
21	26	8	0	38	46	30,04	85		NE	1	Fine.
	35	2	0	35	50	30,02	81		NE	1	Fine.
22	33	8	0	34	46	29,84	83		NNE	2	Cloudy.
	35	2	0	35	49	29,60	82		NNE	2	Cloudy.
23	29	8	0	32	46	29,57	88		ENE	1	Snow.
	35	2	0	35	48	29,64	90		S	1	Cloudy.
24	19	8	0	20	44	29,76	88		E	1	Fair.
	32	2	0	30	47	29,72	88		NE	1	Fair.
25	30	8	0	35	45	29,63	90		NE	1	Cloudy.
	38	2	0	38	47	29,55	91		E	1	Cloudy.
26	35	8	0	35	45	29,47	93	0,130	E	1	Cloudy.
	37	2	0	37	48	29,47	93		NE	1	Cloudy.
27	33	8	0	34	45	29,55	91	0,075	NE	1	Cloudy.
	37	2	0	37	48	29,60	90		NE	1	Cloudy.
28	34	8	0	34	45	29,69	84		NE	2	Cloudy.
	35	2	0	34	48	29,68	82		NE	2	Cloudy.
29	31	8	0	32	45	29,80	80		NE	2	Cloudy.
	34	2	0	33	47	29,96	77		NE	2	Cloudy.
30	27	8	0	27	44	30,18	81		NE	1	Fine.
	32	2	0	32	47	30,16	82		ENE	1	Fine.
31	28	8	0	32	44	30,06	92		ENE	1	Fine.
	36	2	0	36	48	30,01	74		ENE	2	Fine.

1804.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	55	27	45,0	55	28	45,5	59	47	54,4	30,18	28,68	29,60	97	77	88,8	1,673
February	51	25	38,9	51	26	38,9	59	48	51,7	30,51	29,05	30,03	95	69	83,0	1,337
March	62	28	43,2	62	28	43,4	60	48	53,7	29,92	29,15	29,63	96	68	83,9	1,539
April	71	34	46,3	70	36	46,8	61	52	55,5	30,28	29,22	29,71	94	65	81,0	1,604
May	73	44	59,6	73	42	60,2	67	60	62,7	30,29	29,64	29,92	92	64	76,7	1,248
June	87	47	64,7	85	50	64,3	71	61	65,9	30,40	29,68	30,11	88	62	75,6	0,522
July	80	49	62,8	79	53	63,6	69	62	65,8	30,23	29,44	29,84	94	60	75,6	3,705
August	81	49	63,2	81	50	63,2	70	62	65,4	30,30	29,18	29,88	93	62	76,2	2,801
September	81½	45	61,7	81	47	62,2	74	60	66,8	30,47	29,88	30,13	91	63	77,5	
October	68	38	53,6	68	38	53,8	63	57	60,3	30,16	29,18	29,70	95	68	83,4	2,046
November	59	34	45,9	58	34	46,3	62	50	57,0	30,27	29,22	29,84	94	73	85,7	3,993
December	51	19	37,1	50	20	37,5	58	44	51,4	30,40	29,06	29,80	95	74	86,9	0,505
Whole year			51,8			52,1			59,2			29,85			81,2	20,973

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

Variation of the Magnetic Needle,
1804.

March	24°. 11′.8
June	24°. 8′.4
July	24°. 10′.8
September	24°. 11′.3
December	24°. 11′.4

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCV.

PART II.

LONDON,

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PHILOSOPHICAL

TRANSACTIONS.

IX. *Abstract of Observations on a diurnal Variation of the Barometer between the Tropics. By* J. Horsburgh, *Esq. In a Letter to* Henry Cavendish, *Esq. F. R. S.*

Read March 14, 1805.

SIR, Bombay, April 20, 1804.

When I was in London at the conclusion of the year 1801, I had the pleasure of being introduced to you by my friend Mr. Dalrymple, at which time he presented you with some sheets of meteorological observations, with barometer and thermometer, made by me in India, and during a passage from India to England.

Being of opinion that few registers of the barometer are kept at sea, especially in low latitudes, I have been induced to continue my observations since I left England, judging that, even if they were found to be of no utility, they might at least be entertaining to you or other gentlemen, who have been making observations of a similar nature.

During my last voyage I have employed two marine barometers, one made by TROUGHTON, the other by RAMSDEN, and a thermometer by FRAZER. These were placed exposed to a free current of air in a cabin, where the basons of the barometers were 13 feet above the level of the sea.

The hours at which the heights of the barometers, and thermometers were taken, *viz.* noon, IV hours, X hours, XII hours, XVI hours, and XIX hours, were chosen, because at these times the mercury in the barometer had been perceived to be regularly stationary between the tropics, by former observations made in India in 1800 and 1801. It was found that in settled weather in the Indian seas, from 8 AM to noon, the mercury in the barometer was generally stationary, and at the point of greatest elevation; after noon it began to fall, and continued falling till 4 afternoon, at which time it arrived at the lowest point of depression. From IV or V PM the mercury rose again, and continued rising till about IX or X PM, at which time it had again acquired its greatest point of elevation, and continued stationary nearly till midnight; after which it began to fall, till at IV AM it was again as low as it had been at IV afternoon preceding; but from this time it rose till 7 or 8 o'clock, when it reached the highest point of elevation, and continued stationary till noon.

Thus was the mercury observed to be subject to a regular elevation and depression twice in every 24 hours in settled weather; and the lowest station was observed to be at about 4 o'clock in the morning and evening. I remarked that the mercury never remained long fixed at this low station, but had a regular tendency to rise from thence till towards 8 in the morning and about 9 in the evening, and from those times continued stationary till noon and midnight.

In unsettled blowing weather, especially at Bombay during the rains, these regular ebbings and flowings of the mercury could not be perceived; but a tendency to them was at sometimes observable when the weather was more settled.

In the sheets, which I formerly presented to you, were

within the tropics, in steady weather, as had been observed by Mess. CASSAN and PEYROUSE, by Dr. BALFOUR of Calcutta, and others. But since my last arrival in India, I have observed that the atmosphere appears to produce a different effect on the barometer at *sea* from what it does on *shore*.

As I am ignorant whether this phenomenon has been noticed by any person before, I will here give you an abstract of my journal, shewing how the barometer has been influenced during the whole time since I left England, which will enable you to form an idea whether I am right in concluding that the barometer is really differently affected at sea from what it is on shore, at those places in India where the observations have been made.

The first sheet begins with the observations made on board ship, in my voyage from London towards Bombay, in the months of April and May, 1802.

From the time of leaving the Land's End, April 19th, the motion of the mercury in barometers was fluctuating and irregular until we were in latitude 26° N, longitude 20° W, on April 29th; the mercury in barometers then became uniform in performing two elevations and two depressions every 24 hours, (which for brevity in mentioning hereafter I will call equatropical motions.) From latitude 26° N to latitude 10° N, the difference of the high and low stations of the mercury in the barometers was not so great, as it was from latitude 10° N across the equator, and from thence to latitude 25° S. Within these last-mentioned limits, the difference of high and low stations of the mercury in the barometers was very considerable, generally from five to nine hundered parts of an inch, both in the daily and nightly motions.

When we reached the latitude of 28° S, longitude 27° W, June 7th, the mercury in barometers no longer adhered to the equatropical motions; but then, as in high north latitudes, its rising and falling became irregular and fluctuating during our run from latitude 28° S, longitude 27° W, (mostly between the parallels of 35° and 36° S,) until we were in latitude 27° S, and longitude 51° E, on the 11th of July. The mercury then began to perform the equatropical motions, and continued them uniformly, during our run from the last-mentioned position, up the Madagascar Archipelago, across the Equator, until our arrival at Bombay July 31st, 1802.

August 6th, 1802. When the barometers were placed on shore in Bombay, the mercury, for the first six days, appeared to have a small tendency towards performing the equatropical motions, but not equally perceptible as when at sea, the difference between the high and low stations of the mercury in the barometers being great to the day we entered the harbour of Bombay. From the 12th of August to the 22d the mercury could not in general be observed to have any inclination to perform the equatropical motions, although at times a very small tendency towards performing them might be perceived.

On the 23d of August the barometers were taken from the shore to the ship. Immediately on leaving Bombay harbour, August 26th, 1802, the mercury in the barometers performed the equatropical motions, and continued them with great uniformity, during our passage down the Malabar coast, across the bay of Bengal, in the Strait of Malacca, and through the China Sea, until our arrival in Canton river on the 4th of October. When in the river, the mercury became nearly stationary during the 24 hours, except a small inclination at times towards the equatropical motions, but they were not near so perceptible as at sea; this change taking place the day we got into the river.

During our stay in China, the barometer on shore, at Canton, had very little tendency towards the equatropical motions, throughout the months of October and November that we remained there. At times, while in China, a small inclination towards performing the equatropical motions appeared: but, as in Bombay, the difference of rise and fall was of so small a quantity, as to be frequently imperceptible.

December 2d, 1802. On our departure from Canton river, the equatropical motions were instantly performed by the mercury, and with great regularity continued during the whole of the passage to Bombay, until our arrival in that harbour on the 11th of January, 1803.

On January 18th, the barometers were placed on shore, and did not appear in the smallest degree subject to the equatropical motions; although, with great regularity, they had been performed while at sea, even to the day we entered the harbour. One of the barometers was left on board for a few days, and, like that on shore, seemed to have no tendency towards the equatropical motions. During the months of February and March, in Bombay, the mercury was nearly stationary throughout the 24 hours. But about the latter part of March the mercury seemed to incline towards the equatropical motions in a very small degree; and, during the month of April, and to the 20th of May, this small tendency of the mercury to perform the motions appeared at times, but was hardly discernible, the rise and fall being of so small a quantity. From the 18th of January to the 20th of May, the mercury in the barometers was in general stationary, except a very small tendency towards the equatropical motions at times. At other times some change in the atmosphere disturbed the mercury from its stationary position; but this was seldom the case, as it was then the fair weather season, or north-east monsoon.

We sailed from Bombay on the 23d of May, 1803. The instant we got out of the harbour, the mercury in the barometers conformed to the equatropical motions with great regularity, and the difference between the high and low stations was very considerable during the whole of the passage to China, excepting a few days in the eastern parts of Malacca Strait, where the land lay contiguous on each side of us; the difference between the high and low stations of the mercury was then not so great as in the open sea. On clearing the Strait, and entering the China Sea, the equatropical motions were performed in greater quantity, and continued regular during our passage up the China Sea, until July 2d, 1803. We then entered Canton river, and the equatropical motions of the mercury in barometers entirely ceased.

From July 8th to September 7th, the barometers were placed on shore in Canton, during which time the mercury appeared to have no tendency towards performing the equatropical motions; but it inclined to a stationary position, except when influenced by changes of weather. After the barometers were taken from Canton to the ship, we were four days in getting clear of the river, in which time the mercury inclined to be stationary, excepting that a small inclination towards the equatropical motions seemed to evince itself at times. But no sooner had we cleared Canton river, September 13th, 1803, than the mercury in the barometers began to conform to the equatropical motions, of two elevations and two depressions every 24 hours, at equal intervals of time, (although we were near the land until the 15th September.) And the mercury, with great regularity, continued to perform the equatropical motions, from September 13th, 1803, the day we cleared the river of Canton, until October 13th, when we entered Sincapore Strait, excepting a small degree of irregularity, which affected the mercury on the 22d September, when it blew a gale on the coast of Isiompa.

October 13th, 1803. On entering the Strait of Sincapore, which is about 3½ leagues wide, the mercury in the barometers was then a little obstructed, and did not perform the equatropical motions, in the same quantity of rise and fall, as when we were in the China Sea. But on the following day, October 14th, when we had passed the narrow part of the Strait, the mercury conformed to those motions with regularity until October 21st, when we arrived in the harbour of Prince of Wales's Island; then a great retardation took place in the equatropical motions; for, during the time the ship remained in the harbour, from October 20th to November 5th, 1803, the mercury in barometers seemed only in a small degree subject to them, the difference between the high and low stations of the mercury, being in general not more than half the quantity, that takes place in the open sea, or at a considerable distance from land. Where the ship lay at this time in the harbour, the land, on one side, was a full quarter of a mile distant, and on the other side about 1½ mile.

On November 5th, being clear of the harbour of Prince of Wales's Island, the equatropical motions were instantly performed by the mercury, in the usual quantity experienced at sea; which continued with uniformity until December 3d. On this, and the following day, the mercury fell considerably during our passage over the tails of the sands at the entrance of Hoogly river, in latitude 21° 06′ N; and on December 5th, the day of the moon's last quarter, a gale of wind commenced from NNE, with much lightning and rain in the night. During the latter part of this day, the mercury began to rise, and there soon followed a change of settled weather. When we were in the lower part of the river, the mercury appeared to conform in a small degree to the equatropical motions; but when well up the river, at Diamond Harbour, the mercury inclined to be nearly stationary during the 24 hours, as has formerly been observed to happen in Canton river, Bombay harbour, &c.

On January 13th, 1804, after we had cleared the river Hoogly, the mercury in the barometers began to perform its motions with uniformity, which continued during the passage to Bombay, until our arrival there on February 12th. The barometers being then placed on shore, the mercury inclined to a stationary position, without evincing any propensity towards the equatropical motions from the 12th to the 18th February, 1804, as has been noticed in the foregoing description, to happen frequently, on entering a harbour from sea.

On February 18th, 1804. The meteorological journal ceases, at which time it comprises the observations of 22 months, having commenced April 6th, 1802, in Margate Road.

I have taken the liberty of sending you this abstract from the journal, to exhibit the apparent difference of the mercury in the barometer at sea, from what has been observed on shore, at those places mentioned in the preceding description. As I have not seen any account indicating the phenomenon, I thought it might be interesting to you, or other gentlemen of the Royal Society to forward this imperfect abstract, the journal itself being too cumbersome to send home at present. But as I am in expectation of returning to England by the ships from China next season, I hope I shall be enabled to present you with the meteorological sheets alluded to above.

I am, &c.

J. HORSBURGH.

P. S. Since I wrote the foregoing abstract, I have received a letter from my friend Mr. DALRYMPLE, intimating that a copy of the meteorological journal itself would be acceptable, which has induced me to transmit to him the original sheets, with a request to deliver them to you. I regret that I could not find leisure time to make out a fair copy, to have sent to you, in place of the original sheets in their rough state.

Bombay,
June 1st, 1804.

X. *Concerning the Differences in the magnetic Needle, on Board the Investigator, arising from an Alteration in the Direction of the Ship's Head. By* Matthew Flinders, *Esq. Commander of his Majesty's Ship Investigator. In a Letter to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read March 28, 1805.

WHILST surveying along the south coast of New Holland, in 1801 and 1802, I observed a considerable difference in the direction of the magnetic needle, when there was no other apparent cause for it than that of the ship's head being in a different direction. This occasioned much perplexity in laying down the bearings, and in allowing a proper variation upon them, and put me under the necessity of endeavouring to find out some method of correcting or allowing for these differences; for unless this could be done, many errors must unavoidably get admission into the chart. I first removed two guns into the hold, which had stood near the compasses, and afterwards fixed the surveying compass exactly a-midships upon the binnacle, for at first it was occasionally shifted to the weather side as the ship went about; but neither of these two arrangements produced any material effect in remedying the disagreements.

The following Table contains the observations for the variation of the compass in which the differences are most remarkable, and from which I shall beg to point out such inferences as I think may be drawn from them.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation	Ship's head.	Observer.
1801.									
Dec. AM	35° 5′ S	116° 28′ E	two	4 azimuths	binnacle	7° 0′ W	5° 59′ W	NW b. N	Commander
	Princess Royal Harbour		three	6 —	on shore	6 15	6 23	—	
	—	—	theodolite	1 —	—	—	6 8	—	
1802.									
Jan. 9, PM	34 1	121 20	one	2 —	binnacle	5 0	9 22	ESE	
16, AM	Goose Island	Bay	one	2 —	—	4 0	0 54	W	
1803.									
May 20, AM	—	—	—	2 —	—	—	6 8	E	
1802.									
Jan. 18, PM	33 37	124 10	—	2 —	—	4 30	5 44	NNE	
20, PM	32 38	125 35	—	2 —	—	—	7 15	E b. N	
21, PM	32 30	125 48	—	2 —	—	—	4 45	S	
22, PM	32 30	126 7	—	2 —	—	4 15	6 13	NE b. E	
23, PM	32 21	126 33	—	1 amplitude	—	4 0	4 18	S b. E	
24, AM	32 5	128 15	—	2 azimuths	—	3 0	6 4	E b. N	
26, PM	32 15	128 15	three	6 —	—	—	3 7	S b. E	Lt. Flinders
30, AM	32 18	132 29	one	2 —	—	0 30	1 41	SSE	Commander
Feb. 4, AM	No. 4, bay	in island e	—	2 —	—	0 15	2 23	Easterly	
5, AM	32 39	133 55	—	1 amplitude	—	—	1 56	E b S	
6, AM	32 36	133 58	three	6 azimuths	—	—	1 0 E	NW	Lt. Flinders
16, PM	34 3	135 20	one	2 —	—	1 5 E	1 33 W	SE b. E	Commander
—, PM	34 5	135 24	—	1 amplitude	—	1 5	3 56 E	SW	
18, AM	34 50	135 32	three	6 azimuths	—	1 12	1 12	S	Lt. Flinders
Mar. 1, PM	In No. 10,	bay	theodolite	1 —	on shore	1 39	1 39	—	Commander
5, PM	—	—	one	2 —	binnacle	1 39	0 53	S b. E	
17, PM	34 12	137 20	—	1 amplitude	—	2 15	4 38	SW b. S	Commander
18, PM	34 23	137 36	one	2 azimuths	binnacle	2 15	0 35	SE	
21, AM	35 33	137 15	three	6 —	—	2 40 E	1 10	SE b. S	
23, AM	Kanguroo	Island	two	4 —	—	2 58	6 31	SSW	
26, AM	35 10	137 41	one	1 amplitude	—	2 45	1 49	NE b. N	
27, AM	35 21	137 52	one	1 —	—	2 50	1 49	SSE	
April 6, AM	Kanguroo	Island	—	2 azimuths	on shore	2 58	2 58	—	
10, AM	35 47	139 15	—	1 amplitude	binnacle	3 0	5 11	W b. S	
11, PM	35 53	139 26	—	2 azimuths	—	3 0	0 50	SE	
13, PM	36 45	140 5	—	2 —	—	3 30	1 25	SE b. S	
16, AM	37 55	139 55	—	2 — 1 amplitude	—	4 15	2 20	—	
17, PM	37 57	139 56	—	2 azimuths	—	4 15	2 2	NE	
22, AM	39 38	144 50	—	2 —	—	7 45	11 52	WSW	
26, PM	38 35	144 25	—	2 —	—	7 30	3 41	NE b. E	
—, AM	38 38	144 35	—	1 amplitude	—	7 30	6 48	NE b. N	

Note. All the compasses made use of on board the Investigator were of WALKER's construction, one excepted, which was made by ADAMS, and used only on July 22d, 1801.

It is apparent that some of the observed variations in the above Table are 4° less and others 4° greater than the truth; and it may be remarked, that when this error is westward, the ship's head was east, or nearly so, and when it was eastward the head was in the opposite direction. When the observations agree nearest with what was taken on shore, or with what may be deemed the true variation, the ship's head was nearly north or south; and a minute inspection of the Table will favour the opinion, that the excess or diminution of the variation was generally in proportion as the ship's head inclined on either side from the magnetic meridian.

After I had well ascertained the certainty of a difference in the compasses, arising from an alteration in the point steered, I judged it necessary, when I wanted a set of bearings from a point where we tacked the ship, to take one set just before and another immediately after that operation: some specimens of these here follow.

		Head ESE.		*Head* SW b. W.
1802. April 13th	Le Geographe Rocks,	N 55° to 71° E		
11^h 32', AM	♊ point	N 4 W	after tacking	N 9° W
—	Π point	S 32 E		S 40 E.

		Head SE b. E.		*Head* W.
April 14th	Π point rocky, inner part	N 39° E	after tacking,	N 30° E
9^h 29', AM	——— projecting part	N 67 E		N 59 E
—	Furthest visible extreme from deck	S 51 E		S 55 E.

		Head ENE.		*Head* SW b. S.
April 15th	Π, the western part	N 15° W	after tacking,	N 21° W
11^h 50', AM	A peaked hummock	N 19 E		N 15 E
—	Furthest extreme from deck	S 53 E		S 61 E
—	Centre of a naked sandy patch	E		E 5 N.

Variation per amplitude April 15, AM, taken with the surveying compass } 4° 8′ E, ship's head being S.

		Head E.		*Head* SW b. S.
April 15th, 5^h PM	The peaked hummock	N 12° W	after tacking,	N 18° W
	Former extreme, a projection	S 59 E		S 64 E
—	Naked sandy patch, distant 3½′	N 33 E		N 31 E.

From some little change of place after tacking the ship, and from the part whose bearing was set not being perhaps the individual spot in both instances, the difference between the separate bearings in any set will not be always the same: to these causes for error also may be added inaccuracies in taking the angles arising from the motion of the ship and compass, from the view of the object being obstructed by the rigging, masts, or ship's upper works, and from too much haste to get the bearings before the ship's place was materially altered. Even in the Table of azimuths and amplitudes greater accuracy than one degree must not be looked for; and in ship-bearings 2 or even 3 degrees is not, I believe, too great an allowance for error, unless in very favourable circumstances.

Without attending to small differences, it is evident that the bearings correspond with the observations in requiring a less east variation to be applied when the ship's head was easterly, and a greater when it was to the westward, in order to get at the true direction of the object.* When examining the north

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* As a specimen of the plan I followed in protracting such bearings as the above, take the set of April 15, AM, when the true variation appears to have been 4° E. On the first bearings the ship's head was six points on one side of the meridian, and on the second it was three points on the other side, the mean is one point and an half on the east side; now for this one point and an half I allow 1° of error, which, as it is on the east side of the meridian, and the variation is easterly, must be subtracted: the variation then to be allowed upon the mean between the bearings before and after tacking will be 3° E, from which the true bearings will stand as follows:

April 15th, AM $11^h\ 50'$	Π western part - - - -	N 15° E
	A peaked hummock - - - -	N 20 E
—	Furthest extreme from deck - - -	S 54 E
—	Centre of a naked sandy patch - -	E 0½ S.

In the same manner upon single sets of bearings I was obliged to allow a variation different from what I supposed the true to be, unless the ship's head was nearly north or south: but, that I might proceed as little upon conjecture as possible, I always and east coasts of New Holland, I always endeavoured to take the angles on shore with a TROUGHTON's portable theodolite, and to observe for the variation in the same places, that all the errors might be done away or corrected; and as I was frequently fortunate enough to carry on my surveys in this manner for weeks together, instances that might corroborate or contradict the preceding remarks are neither very numerous or pointed; the following are the most remarkable.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1802.	° ′	° ′				° ′	° ′		
Aug. 5, PM	23 51 S	151 42 E	one	1 amplitude	binnacle	8 0 E	12 7 E	WSW	Commander
— AM	23 51	151 40	—	—	—	—	10 15	WNW	
12, PM	23 30	151 11	three	6 azimuths	—	—	6 50	SSE	Lt. Flinders
18, PM	23 23	151 16	one	2 —	—	7 45	7 52	W	Commander
31	22 23	150 38	two	4 —	—	7 30	4 49	E	
Sept. 6, AM	Upon Pier	Head	theodolite	1 —	on shore	8 0	8 2	—	
Oct. 14, PM	20 44	150 42	one	1 amplitude	binnacle	7 0	6 40	SSE	Lt. Flinders
20, PM	19 22	148 40	—	1 —	—	6 0	5 39	S	Commander
21, AM	18 15	148 38	three	6 azimuths	—	—	5 42	N b. E	Lt. Flinders
Nov. 2, PM	10 30	142 32	one	2 —	—	4 0	3 32	E	Commander
7, AM	12 11	142 0	—	2 —	—	—	4 4	S	Lt. Flinders
9, PM	12 37	142 2	—	1 amplitude	—	—	5 24	W	Commander
1803.									
Jan. 3, PM	14 20	136 16	—	1 —	—	2 30	0 58	E	
7, PM	14 20	136 37	—	1 —	—	—	1 9	SE	
13, {PM AM}	13 38	137 20	—	2 —	—	3 0	3 47	Westerly	Lt. Flinders
14, AM	13 35	136 58	—	1 —	—	—	5 51	WSW	Commander
16, PM	In NW Bay	(Gr. Eyl.)	theodolite	1 azimuth	on shore	—	3 6	—	
Feb. 3, AM	Arnhem S	Bay	three	6 —	binnacle	2 20	2 26	NW b. W	
9, AM	—	—	theodolite	1 —	on shore	—	2 20	—	
Mar. 10, AM	11 5	134 15	one	2 —	binnacle	1 0	1 55	WNW	

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endeavoured to get observations for the variation when the ship's head was in the same direction as when I had taken or wished to take a particular set of bearings, and I then allowed that variation exactly, whatever it was. The perplexity arising from disagreements in bearings was by these means much alleviated, and happy agreements were frequently produced, when, without such corrections, there was nothing but discord.

In the latter of these observations, the differences arising from a change in the direction of the ship's head is less considerable than in the higher latitudes; indeed, on approaching the line of no variation upon the south coast, the differences in the variation were smaller than before and afterwards; but that these differences shall be greater in a large variation and smaller in a less, both places being equally distant from the magnetic pole, I will not venture to assert. The inferences that I think may be safely drawn from the above observations are as follows: 1st. That there was a difference in the direction of the magnetic needle on board the Investigator when the ship's head pointed to the east, and when it was directed westward. 2d. That this difference was easterly when the ship's head was west, and westerly when it was east. 3d. That when the ship's head was north or south the needle took the same direction or nearly so that it would on shore; and shewed a variation from the true meridian, which was nearly the medium between what it showed when east and when west. 4th. That the error in variation was nearly proportionate to the number of points which the ship's head was from the north or south. Constant employment upon practice has not allowed me to become much acquainted with theories, but the little information I have upon the subject of magnetism has led me to form some notion concerning the cause of these differences, and although most probably vague and unscientific, I trust for the candour of the learned in submitting it, as well as the inferences above drawn, to their judgment.

1st. I suppose the attractive power of the different bodies in a ship, which are capable of affecting the compass, to be collected into something like a focal point or center of gravity, and that this point is nearly in the center of the ship where the shot are deposited, for here the greatest quantity of iron is collected together.

2d. I suppose this point to be endued with the same kind of attraction as the pole of the hemisphere where the ship is; consequently, in New Holland the south end of the needle would be attracted by it and the north end repelled.

3d. That the attractive power of this point is sufficiently strong in a ship of war to interfere with the action of the magnetic poles upon a compass placed upon or in the binnacle.

If these suppositions are consistent with the laws of magnetism, established by experiments, I judge that they will account for all the differences above noticed; for the interference will necessarily be most perceptible upon a compass when the attractive point is at right angles to the magnetic meridian, that is, when the ship's head is east or west, and will altogether vanish or become imperceptible when the attractive point and meridian coincide, or when the ship's head is north or south. That the power of this point should become less as the ship increases her distance from the magnetic pole has not indeed entered into my suppositions; but it may probably be true, and is indeed almost a necessary consequence of the second supposition. If the above hypothesis, so to call it, be true, it must follow, that the differences in the variation of the magnetic needle, arising from a change in the ship's head, ought to be directly contrary to those before recited, when the ship is on the north side of the magnetic equator, for the north point of the needle should then be attracted, and the south end repelled. I have no observations which are very decisive upon this head, but those that were taken on board the Investigator seem to bespeak that it is so; they are as

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1801.									
July 21, PM	Start Point in sight to the NE		two	5 azimuths	binnacle	—	29° 34′ W	W	Mr. Thistle
—			one	1 amplitude	—	—	29 30	—	—
22, PM	49° 10′ N	5° 25′ W	two	4 azimuths	upon the booms in the middle of the ship	—	24 12	WNW	—
—, AM	48 15	6 45	one	1 amplitude		—	24 49	WSW	—
28, PM	38 1	14 20	five	10 azimuths		—	20 57	SW	—
—	—	—	—	11 —	binnacle	—	25 34	—	Commander
31, PM	Porto Santo in sight to the NW		two	4 —	—	—	22 45	—	Mr. Thistle
—			—	4 —	booms	—	19 51	—	—
Aug. 24, AM	10 20	22 15	one	2 —	binnacle	—	12 45	SE b. S	Commander
29, AM	5 40	16 30	two	4 —	—	—	12 18	—	Lt. Flinders
Sept. 5, AM	2 15	14 00	—	3 —	—	—	14 54	WSW	Mr. Thistle

These observations, particularly those of July 28, seem to be decisive in showing that the variation is more westerly when taken upon the binnacle of a ship whose head is westward in north latitude, than when observed in the center of the ship, which is a strong confirmation of the suppositions before given; but the observations on the change of the ship's head are too few to be satisfactory. Almost every sea officer can tell whether he has observed the variation of the compass to be greater when going down the English Channel than when coming up it: and indeed it would be very easy for a ship lying in harbour to ascertain the point beyond controversy. Should this point be well established, I think it would follow, that from a high south latitude where the differences are great on one side, they are most likely to decrease gradually to the equator, and to increase in the same way to a high north latitude, where they are great on the other side; thus the smaller differences on the north coast of New Holland will be accounted for. I shall leave it to the learned on the subject of magnetism to compare the observations here given with those made by others in different parts of the earth, and to form from them an hypothesis that may embrace the whole of the phenomena: the opinion I have ventured to offer is merely the vague conjecture of one who does not profess to understand the subject. Some account of the magnetism of Pier Head, upon the east coast of New Holland, may not perhaps be thought an unappropriate conclusion to this Paper. I was induced to attend to this from the following passage in Hawkesworth, Vol. III. p. 126. " At sun-rise I went ashore," says Captain Cook, " and " climbing a considerable hill," Pier Head, " I took a view of " the coast and the islands that lie off it, with their bearings, " having an azimuth compass with me for that purpose; but I " observed that the needle differed very considerably in its " position, even to thirty degrees, in some places more, in " others less; and once I found it differ from itself no less " than two points in the distance of fourteen feet.* I took up " some of the loose stones that lay upon the ground, and " applied them to the needle, but they produced no effect;

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* In a set of angles taken near the head of Arnhem north bay, on the west side of the gulph of Carpentaria, I found the needle of the theodolite had been drawn 50° from its proper direction. The shore consisted of grains of iron ore caked into a stony mass; and a piece of it, when applied to the needle, drew it 6 or 8 degrees from its direction, but it then swung back to its error of 50° where it was stationary. In Arnhem south bay a small piece of similar stone drew the needle of the theodolite entirely round, yet the bearings taken in this place did not show any disagreement from the variation and bearings taken in the neighbouring places, where the stone did not produce any such effect. In most places on shore, where I had occasion to take angles, it was my practice to try the effect of a piece of the stone upon the theodolite, in order to detect the presence of iron ore, as well as on account of my survey. It

" and I therefore concluded that there was iron ore in the hills, " of which I had remarked other indications, both here and " in the neighbouring parts."

On landing at Pier Head I found the stones lying on the surface to be porphyry, of a dark bluish colour; but although I understand this species is usually found to possess some magnetic power, a piece did not produce any sensible effect upon the needle of the theodolite when applied to it. In the following observations the theodolite always stood about four feet from the ground, that being nearly the length of its legs. I first took an extensive set of bearings from the top of the hill, amongst which were two stations whence Pier Head had been before set. The first, called Extensive Mount, distant 34 miles, differed from its back bearing 4° 35′ to the right, and the second, island *a*, distant 29½ miles, differed 4° 45′ the same way. I now moved the theodolite three yards to the westward, and the same two objects bore 2° 10′ to the right of their back bearing; on moving it three yards to the south-eastward from the first place, they differed 2° to the left; and on moving the theodolite four yards to the northward, the same two objects bore 1° 10′ to the right of their back bearings. On the following morning I determined to try the magnetism more particularly. Taking the theodolite and dipping-needle, I landed upon the shore of the Head, whence the top of the hill bore N 50° W, about one-third of a mile. The variation of the theodolite in this place I observed to be 8° 2′ E, and the

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commonly happened that no effect was apparent, but yet I could not trust implicitly to the angles, (particularly on the main land,) unless observations for the variation were taken before the instrument was moved, or I had a back bearing of some station where such observations had been made.

inclination of the south end of the dipping needle 50° 50′, the needle stood vertical when the face of the instrument was S 2° E. I then took the following bearings: Extensive Mount 108° 30′, the same exactly as by back bearing. Double Peak 143° 30′; from hence I rowed round the Head, and landed on a rock, whence the top of the hill bore SSW one-sixth of a mile; Extensive Mount bore 110° 14′, the inclination of the dipping-needle 50° 29′, and the needle stood vertical when the instrument faced S 3° E. Thus the difference was 1¾° in the horizontal, and ½° in the vertical direction of the needle. Ascending the hill, I made the following observations on the top: Extensive Mount 113° 50′, *a* island 133° 52′, Double Peak 148° 32′; the inclination of the needle was 53° 20′, and it stood vertical at S 3° E. The differences here are 5° 10′ in the horizontal, and 2° 30′ in the vertical direction, from what the needle stood at in the first morning's place. On moving ten yards SSE, the bearings were, Extensive Mount 108° 44′, Double Peak 143° 25′; the inclination was 52° 18′, and the needle was vertical when the instrument faced S 5° W. In this 4th set of observations, the horizontal direction of the needle is only a few minutes different from the first place, but the vertical direction is 1° 28′. From the top of the hill I now moved twenty yards to the north-eastward, when Extensive Mount bore 110°, Double Peak 144° 42′; the inclination of the dipping needle was now 50° 35′, and it stood vertical at S 3° W. Thus it appears that the polarity of the magnetic needle is most interrupted at the top of the hill, both according to the theodolite and dipping-needle. Whether this may arise from some particular magnetic substance lodged in the heart of the hill, or from the attractive powers of all the substances which compose Pier Head being centered in a similar point to what I have supposed to take place with all the ferruginous bodies lodged within a ship, I shall not attempt to decide. The greater differences in the horizontal direction of the needle observed by Captain Cook, might have arisen from his using a common azimuth compass, which was probably not further elevated from the ground than to be placed on a stone.

MATTHEW FLINDERS.

Isle of France,
March 5th, 1804.

XI. *The Physiology of the Stapes, one of the Bones of the Organ of Hearing; deduced from a comparative View of its Structure, and Uses, in different Animals. By* Anthony Carlisle, *Esq. F.R.S.*

Read April 4, 1805.

Anatomical descriptions of the mechanism of the eye have importantly contributed to the advancement of optics, a branch of science which has conferred numerous benefits on mankind. Whether a more intimate knowledge of the structure of the organs of hearing may illustrate the doctrines of acoustics, and thus become a source of similar advantages, can only be determined by future investigations, and experiments. The following is an attempt to exhibit a part of the instrument of hearing, taken from several orders of animals, with an intention to shew the office it holds, and the relation it bears to other parts of the auditory mechanism. The minuteness of this research will not require any apology to that learned Body, who for a long series of years have witnessed the dependance of all the systems of natural knowledge on simple particulars, well chosen, and applied to the establishment of general laws.

Doubtless the whole organ of hearing is an apparatus to collect occurring sounds, and to convey them to the seat of that peculiar sensation, regulating their intensity, or facilitating their progress, according to the degree of impetus. In these respects the ear resembles the eye.

The ossicula auditûs in man, and in the mammalia, form a series of conductors, through which sounds are transmitted, from the membrana tympani, into the sensitive parts of the organ. The number, forms, and relative junctions of these ossicles are various; but, in all cases, their office seems limited to the conveyance of sounds received through the medium of air; because fishes have no parts corresponding with them. In two classes of animals, the aves, and amphibia, of LINNÆUS, one bone, in the situation of the stapes, is the only ossicle of the tympanum: in all other animals it is placed next to the seat of sensible impression, and most remote from that part of the organ on which sounds first impinge.

The ossicula auditûs are formed of bone, resembling that of teeth; it is close in texture, and brittle: in the growing state, composed of a vascular pulp, the ossification of which is completed soon after birth; and, like the teeth, they cease to grow after that process is finished. The malleus and incus are hollow, and possess an internal periosteum; and the whole series is covered by a reticular membrane which has no red blood-vessels in the adult. It has been asserted by many authors that fat, or marrow, is contained in these bones, but I am induced to attribute their occasional greasy appearance to transudation from the neighbouring parts, during the stage of putrefactive maceration, seeing that all such bones when taken from recent subjects, are free from the marks of fat. Although density seems to be a requisite condition, yet it is convenient that the bones should not be massive, as their figures and relative adaptations evidently show.

The malleus is united to the membrana tympani throughout half its long diameter, by a process called manubrium; its detached end forms a rounded enlargement, which is articulated by a sort of hinge joint to the body of the incus. Three muscles are fixed to the malleus, the most powerful of which draws the manubrium, and membrana tympani perpendicularly inward; the next in strength is inserted upon a slender stem of bone which forms a right angle with the manubrium, and on the plane of the membrana tympani. The smallest mŭscle is fixed to the processus major, pulling the malleus backward, and pressing its head against the joint of the incus. These muscles are all restricted in their actions to the changes produceable on the membrana tympani, because the strong connections of the joints between the malleus and incus, and the incus and stapes, admit of little motion; indeed the former joint is deficient in many animals. The incus has no muscles, and forms only a passive intervention between the malleus and stapes, which last bone has a peculiar muscle appropriated to itself. Hence it appears, that the first series of ossicula audităs has a different office from the stapes, as will be subsequently explained.

The bone, to be now particularly considered, has been called stapes, staffa, stapha, or stapeda, from its resemblance to the stirrup of a saddle. It was first observed about the middle of the sixteenth century; and PHILIP AB INGRASSIAS, REALDUS COLUMBUS, and BARTHOLOMÆUS EUSTACHIUS, have contested the honour of its discovery.

The human stapes is $\frac{6}{40}$ of an inch in height, and $\frac{5}{40}$ in width at its basis: it weighs, when dried, $\frac{1}{32}$ of a grain.

It is divided into the following parts, *viz.*

The capitulum, or articulating head, which joins the os lenticulare.

The collum, which unites the capitulum to the two crura. And

The basis, on which the expanded crura rest and terminate.

The capitulum stapedis has a shallow, concave surface, to receive the os lenticulare, or epiphysis connected to the long leg of the incus. (*Vide* Plate IV. letter *c.*) Around this joint a strong membrane is applied in the manner of a capsular ligament. The capitulum is seldom placed exactly on the top of the Gothic arch formed by the crura, and the crus immediately under the stapedeus muscle, is always the thickest, and most curved. (*Vide* letter *a.*)

The collum is hollow, being only a thin shell of bone; on its side is a small tubercle, to which the tendon of the stapedeus muscle is affixed. See letters *a* and *b*.

The crura are curved, and their interior surfaces are grooved, leaving only a thin osseus plate.

The basis is exactly adapted to the the fenestra ovalis, more properly called fenestra vestibuli by modern anatomists, and the two ends project beyond the crura. The upper surface is generally concave, the under surface slightly convex; and here a rising border marks the insertion of the membrane which connects it to the edges of the fenestra vestibuli. *Vide* letter *c*. The outline of the basis somewhat resembles a long semi-ellipsis, one side being nearly straight, and the other convex. This figure appears adapted to the expansion of the basis, without increasing the bulk of the bone, whilst it gives leverage to the muscle.

When the stapes rests on its basis, with the straight side next to the observer, if the more curved leg be toward the left, then it is the stapes of the right ear; but if on the right, then it is the left stapes. The arch above the straight side of the basis is more rounded than that above the curved side; the latter being an intersection of two curves like the Gothic arch. I have never seen that expansion of membrane across this arch, described by Du Verney; and, from the great number of ears which I have attentively examined, am induced to think that a pellicle of mucilaginous fluid, which often covers the recent bone, has been mistaken for a membrane.

The stapes stands perpendicular to the plane of the membrana tympani; a plane drawn through the crura, parallel to the length of the basis, equally bisects the cavity of the tympanum.

The stapedeus muscle arises within a special cavity in the petrous portion of the temporal bone; it is a short, thick mass of red fibres, covered by fascia; and sends forth a round tendon through a small osseous aperture at the point of the pyramidal eminence, which unites to the collum stapedis in an angle of 50 degrees, toward a line drawn perpendicular to the plane of the basis, and obliquely across its convex side, in an angle of 5 degrees from the bearing of its straight side. The action of the stapedeus muscle is to draw the capitulum downward, and toward the curved side of the basis. This oblique motion depresses the end of the basis under the curved crus, whilst it rotates the incus upon its short leg, and presses its articulation with the malleus into closer contact: but the stapes is not withdrawn from under the long leg of the incus, being prevented by the strong connecting ligaments.

The smaller angle of the tendon crossing the parallel of the crura over the convex side of the basis, necessarily depresses that edge, the straight side acting as a hinge. The externus muscle of the malleus rotates the incus back again, and restores it to its passive perpendicular situation; becoming on such occasions the antagonist of the stapedeus. It is worthy of remark, that all the muscles of the ossicula auditûs act nearly at right angles, or in straight lines, contrary to the ordinary course of muscular application, by which their forces are comparatively augmented.

The varieties in the human stapes are few: they appear in the relative curvature of the crura, and in the degree of slenderness or symmetry of its general form.

The fenestra vestibuli admits the basis of the stapes to pass into the vestibulum, when the connecting membrane is destroyed, there being no other obstacle to its descent.

None of the external similitudes in form, nor any correspondence in the habits, or voices of animals, appear to govern the configuration of these ossicles, except in those mammalia inhabiting the waters, such as the seal, walrus, and whale tribes,* where the stapes is always more massive: but in the otter, which only dives occasionally, the stapes does not vary from that of the fox. In the tiger, dog, and other feræ, the crura are straight, meeting in an acute angle; but the same figure occurs in the horse, beaver, goat, and many more herbivorous quadrupeds; so that no inference can be drawn from these different habits of life.

In the cete, exemplified in the Plate by the porpoise, whose organs of hearing precisely resemble those of whales which I have seen, and agree with the descriptions of others by Professor CAMPER, the muscle of the stapes pulls the capitulum at an angle of 45 degrees, with the plane of the basis, so as remarkably to depress its subjacent end into the fenestra vestibuli; besides the thickness of the basis, and its exact adaptation to the fenestra, exhibit a joint of considerable motion. In those animals there is only a small perforation, instead of the crural arch. *Vide* letter *n*.*

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* I have not had an opportunity of examining the ossicles of a hippopotamus.

I have discovered a very remarkable singularity, in tracing the comparison of this bone, in the marmot, and Guinea-pig. The stapes in these animals is formed with slender crura, constituting a rounded arch, through which an osseus bolt passes, so as to rivet it to its situation. This bolt I have named *pessulus*. *Vide* letter *l*. It is placed near the top of the arch, so that by the action of the stapedeus muscle the upper part of the straight crus is brought into contact with the *pessulus;* and by this means the depression of the basis is limited. It does not seem obvious for what further end this provision is designed, because, excepting the shrill whistle, there is nothing peculiarly different in the habits of those animals from others which are destitute of such mechanism.

The kanguroo has this bone like the corresponding ossicle in birds, called Columella; but it has also the malleus and incus, which birds have not.

In the ornithorhynchus paradoxus, and ornithorhynchus hystrix, the resemblance to the columella is still more striking; and forms an additional point of similarity between these strange quadrupeds and birds. Their columellæ are not, however, articulated to a cartilage, as in birds; but to a small bone performing the office of the manubrium of the malleus.

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* The stapes of the seal has solid rounded crura, and a small aperture; that of the walrus is entirely solid, and the edges as well as the plane of the sides, are a little twisted, agreeing with the observation of M. CUVIER, *Leçons d'Anatomie comparée*, Tome II. p. 505. In all these aquatic mammalia the fenestra rotunda, called also fenestra cochleæ, is large, being three or four diameters more than in other animals of similar bulk.

In birds, a slender bone passes to the fenestra vestibuli, from a cartilage fixed to the membrana tympani: it is called columella, having received that name from JULIUS CASSERIUS.

The capitulum of the columella in birds is slightly expanded, and is joined to an obtuse-angled triangular plate of cartilage, which I have called cartilago columellæ, (*vide* letter *t*,) the longest side of the triangle is attached to the membrana tympani. In some species of birds a small foramen occurs in the middle of this plate, but in many others it is entire.

A strong muscle is inserted into the shorter angle of the cartilage, which draws it downward, and thus elevates the opposite angle in the center of the membrana tympani, so as to render it conical externally. Two lateral ligaments steady the articulation of the cartilage with the head of the columella.

The columellæ in birds are less brittle than the ossicula auditûs in the mammalia; their bases are exactly fitted to the fenestra vestibuli; and that part of those columellæ nearest the base is generally of a reticulated texture.

The amphibia are provided with columellæ, in their form and adaptations resembling those of birds: the cartilage is here, however, united to the under surface of the true skin, without any apparent application of muscles to alter its tension. The substance of the columella is even less hard than in birds; and its basis is considerably smaller than the fenestra vestibuli. The cavity of the tympanum has no lateral cells, and the Eustachian tube is short, and wide, seemingly for the purpose of receiving sounds conveyed through the medium of air.

From the evidence of these facts, together with the comparative view exhibited in the Plate, I am led to the following conclusions. In man, and the most numerous orders of the mammalia, the figure of the stapes is an accommodation to that degree of lightness which, throughout the series of ossicles, seems a requisite condition. It is also a conductor of vibrations in common with the other ossicles: but most especially it is designed to press on the fluid contained in the labyrinth by that action which it receives from the stapedeus muscle, and the hinge-like connection of the straight side of its basis with the fenestra vestibuli; the ultimate effect of which is an increase of the tension of the membrane closing the fenestra cochleæ.

It does not appear that any degree of motion ever subsists between the ossicula auditûs as wholes, which bears any relation to the peculiar vibrations of sounds; but rather that the different motions of these bones only affect the membrana tympani, and alter the degrees of contact in their articulations, so as to influence the intensity of violent impulses; sounds of less impetus, not requiring such modulation, are transmitted through the conducting series by the vibrations of the integrant parts of these bones, unaccompanied by muscular action.

This reasoning is suggested by the columellæ in the aves and amphibia: and as many birds are known to imitate a variety of artificial sounds with great accuracy, it may be inferred that they hear such sounds as acutely, and with the same distinctness as mankind.

It seems that all the muscles of the ossicula auditûs are of the involuntary kind, and the peculiar stimulus to their action is sound. The chorda tympani, which supplies them, is a gangliated nerve: if this supposition be true, then the muscles should be considered as all acting together, and it is well known that persons who hear imperfectly are more sensible to sounds in a noisy place, as if the muscles were by that means awakened to action.

The office which the basis of the stapes holds, and which the stapedeus muscle is especially destined to perform, seems to throw considerable light on the use of the cochlea. It cannot be allowed that the pressure of the watery fluid in the labyrinth is a requisite condition to produce the sensation of hearing, since all birds hear without any mechanism for that purpose, but as such pressure must ultimately give increased tension to the fenestra cochleæ, it follows that we inquire at this part for the principal use of the stapes.

As the membrane of the fenestra cochleæ is exposed to the air contained within the cavity of the tympanum, it appears adapted to receive such sounds as pass through the membrana tympani, without exciting consonant motions in the series of ossicula auditûs.

Experiment.

My head being laid on a table, with the meatus auditorius externus perpendicular to the horizon, my friend, Mr. WILLIAM NICHOLSON, pulled the tragus toward the cheek, and dropped from a small vial, water, at the temperature of my body, into the meatus. The first drop produced a sensation like the report of distant cannon, and the same effect succeeded each following drop, until the cavity was filled.

In this experiment the vibrations of the membrana tympani must have been impaired, if not wholly destroyed, by the contact and pressure of the water; yet the motions of the whole membrane, from the blow of each drop of water, affected the air contained in the tympanum sufficiently to produce a

That something like this occurs in many kinds of sounds is more than probable; and as the cochlea consists of two hollow half cones, winding spirally, and uniting at their apices, it follows that the sounds affecting either the cone terminating in the vestibulum, or that which forms the fenestra cochleæ, must each pass from the wide to the narrow end; and the tension of the parts, in either case, will necessarily aid the impression.

I have already trespassed beyond the usual limits, and must reserve the more ample details of this subject for a work expressly directed to the anatomy and physiology of the organs of hearing.

Explanation of Plate IV.

a, The left* stapes of a human ear magnified two diameters; presenting the curved edge of the basis, and the more elevated and pointed arch.

b, The opposite side of the same stapes, shewing its rounded arch.

c, Two figures, the uppermost being the articulating surface of the capitulum, and the one beneath shewing the under surface of the basis, of the same stapes.

d, Stapes of a hedge-hog, *(Erinaceus Europæus,)* magnified four diameters.

e, Stapes of a mole, *(Talpa Europæa,)* magnified six times.

f, Stapes of the musk ox, *(Bos moschatus,)* twice magnified.

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* The other stapedes are all from the right ears.

g, Stapes of the elephant, *(Elephas maximus,)* natural size.

h, Stapes of the tiger, *(Felis Tigris,)* twice magnified.

i, Stapes of the dog, *(Canis familiaris,)* three times magnified.

j, Stapes of the horse, *(Equus Caballus,)* twice magnified.

k, Stapes of the pig, *(Sus Scrofa,)* three times magnified.

l, Stapes of the marmot *(Arctomys Marmota)* with its pessulus, magnified four times.

m, Stapes of the seal, *(Phoca vitulina,)* twice magnified.

n, Stapes of the porpoise, *(Delphinus Phocæna,)* twice magnified.

o, Stapes of the walrus, *(Trichechus rosmarus,)* natural size.

p, Stapes of the kanguroo, *(Macropus Kanguroo,)* four times magnified.

q, View of the under surface of its basis.

s, Columella of the duck-bill, *(Ornithorhynchus paradoxus,)* magnified four times.

r, Basis of the same columella.

t, Columella and cartilago columellæ of a goose, *(Anas Anser,)* twice magnified.

u, Columella of the Egyptian ibis, *(Tantalus Ibis,)* taken from a mummy, three times magnified.

v, Columella of a turtle, *(Testudo Midas,)* natural size, with its cartilage.

w, Columella of the Gangetic crocodile, *(Lacerta Gangetica,)* natural size.

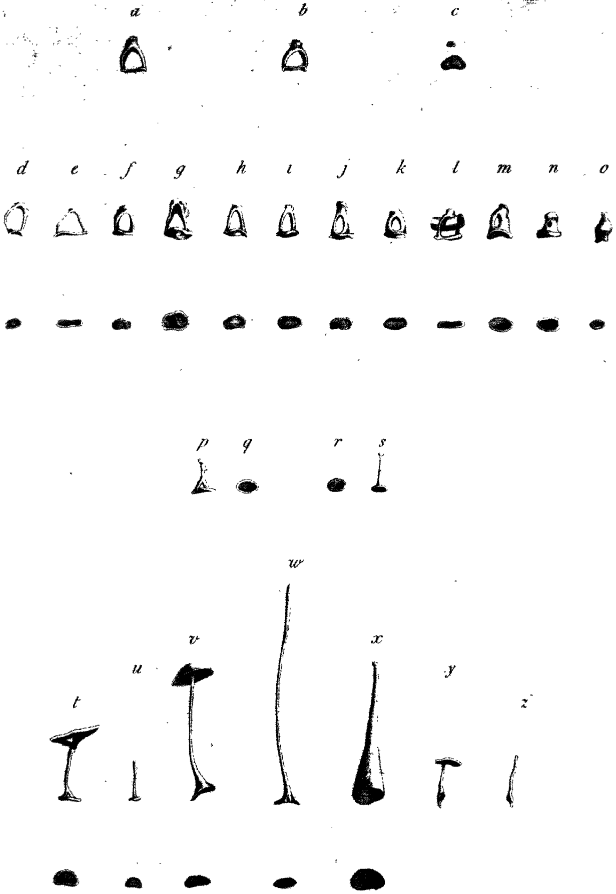
x, Columella of a turtle, *(Testudo coriacea,)* natural size.

y, Columella and cartilage of a frog, *(Rana temporaria,)* twice magnified.

z, Columella of a toad, *(Rana Bufo,)* twice magnified.

The third and last lines of objects in the Plate, exhibit the outlines, and under surfaces, of the bases of the stapedes, and columellæ, immediately above. In some, the surface is convex, in others concave, but neither the one nor the other are constant attendants on any common affinity.

STAPEDES and COLUMELLÆ compared.



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XII. *On an artificial Substance which possesses the principal characteristic Properties of Tannin. By* Charles Hatchett, *Esq. F. R. S.*

Read April 25, 1805.

§ I.

THE discovery of the principle on which the effects of tanning essentially depend, may be partly attributed to Mr. DEYEUX, who obtained a substance from galls which he considered as a species of resin,* but which was afterwards proved by Mr. SEGUIN to be that which renders the skins of animals insoluble in water, and imputrescible, and thus to be the principle by which they are converted into leather.†

The chief characteristic property of this substance was ascertained by Mr. SEGUIN to be that of precipitating gelatine or glue from water in a state of insolubility, and as it was evidently different from any vegetable substance hitherto discovered, he gave it the name of tannin.

This discovery of Mr. SEGUIN at once unveiled the theory of the art; an easy and certain method was afforded by which tannin could be detected, and its relative quantity in different substances be determined, whilst the nature and properties of this newly discovered vegetable principle could be subjected to accurate investigation.

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* *Mémoire sur la Noix de Galle*, par M. DEYEUX; *Annales de Chimie*, Tome XVII. p. 23.

† Ibid. Tome XX. p. 15.

The former has derived elucidation from the experiments of Mr. BIGGIN,[*] and much has been contributed in every respect by Mr. PROUST,[†] but the subject has received the greatest extension and some of the most valuable additions from the ingenious labours of Mr. DAVY, particularly the discovery of the important fact, that catechu or terra japonica consists principally of tannin.[‡]

The united results of the experiments made by these and other eminent chemists, appear therefore to have fully established, that tannin is a peculiar substance or principle which is naturally formed, and exists in a great number of vegetable bodies, such as oak-bark, galls, sumach, catechu, &c. &c. commonly accompanied by extract, gallic acid, and mucilage.

But no one has hitherto supposed that it could be produced by art, unless a fact noticed by Mr. CHENEVIX may be considered as some indication of it.

Mr. CHENEVIX observed, that a decoction of coffee-berries did not precipitate gelatine unless they had been previously roasted; so that tannin had in this case either been formed or had been developed from the other vegetable principles by the effects of heat.[§]

Some recent experiments have however convinced me, that a substance possessing the chief characteristic properties of tannin may be formed by very simple means, not only from vegetable, but even from mineral and animal substances.

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[*] Phil. Trans. 1799, p. 259.

[†] *Annales de Chimie,* Tome XXV. p. 225. Ibid. Tome XLI. p. 331. Ibid. XLII. p. 89.

[‡] Phil. Trans. 1803, p. 233.

[§] Nicholson's Journal for 1802, Vol. II. p. 114.

§ II.

In the course of my experiments on lac, and on some of the resins, I had occasion to notice the powerful effects produced on them by nitric acid, and I have since observed, that by long digestion, almost every species of resin is dissolved, and is so completely changed, that water does not cause any precipitation, and that by evaporation a deep yellow viscid substance is obtained, which is equally soluble in water and in alcohol, so that the resinous characters are obliterated.

When I afterwards had discovered a natural substance, which was composed partly of a resin similar to that of recent vegetables, and partly of asphaltum,* I was induced to extend the experiments already mentioned to the bitumens, in the hope of obtaining some characteristic properties by which the probable original identity of these bodies with vegetable substances might be farther corroborated. In this respect I succeeded, in some measure better than I expected; but I observed a very material difference between the solutions of the resins and those of many of the bitumens, such, for instance, as asphaltum and jet. The first effect of nitric acid, during long digestion with these substances, was to form a very dark brown solution, whilst a deep yellow or orange coloured mass was separated, which by subsequent digestion in another portion of nitric acid was completely dissolved, and by evaporation was converted into a yellow viscid substance, equally soluble in water and in alcohol, so as to perfectly resemble that which by similar means had been obtained from the resins, excepting, that when burned, it emitted an odour somewhat resembling that of the fat oils.

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* Phil. Trans. 1804, p. 385.

It therefore appeared to me, that the first or dark brown solution had been formed by the action of the nitric acid on the *uncombined* carbonaceous part of the bitumens, or that by which they are rendered black, and that the deep yellow portion which was separated, was that which constituted the real or essential part of these bituminous substances. This opinion was confirmed by some experiments which I purposely made upon amber, and having every reason therefore to believe, that the dark brown solution obtained from asphaltum and jet was in fact a solution of coal, I repeated the experiments on several varieties of the pit or mineral coal, from all which, I obtained the dark brown solution in great abundance; but those coals, which contained little or no bitumen, did not yield the deep yellow substance which has been mentioned.

In each experiment I employed 100 grains of the coal, which I digested in an open matrass with one ounce of nitric acid diluted with two ounces of water. (The specific gravity of the acid was 1.40.)

After the vessel had been placed in a sand-bath, and as soon as it became warm, a considerable effervescence attended with much nitrous gas was produced; after about two days I commonly added a second and sometimes a third ounce of the acid, and continued the digestion during five or six days, or until the whole, or nearly the whole, was dissolved, excepting in those cases when the deep yellow substance was formed; for this I constantly separated.

The next experiment was made upon charcoal, which was more readily dissolved than the preceding substances, without leaving any residuum; the solution was perfect, and the colour was reddish-brown.*

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* The solubility of charcoal in nitric acid, and some of its properties when thus

Having thus by means of nitric acid obtained solutions from asphaltum, from jet, from several of the pit-coals, and from charcoal, I evaporated them to dryness in separate vessels, taking care in the latter part of the process to evaporate very gradually, so as completely to expel the remainder of the acid without burning the residuum; this, in every case, proved to be a brown glossy substance, which exhibited a resinous fracture.

The chemical properties of these residua were as follows.

1. They were speedily dissolved by cold water and by alcohol.

2. Their flavour was highly astringent.

3. Exposed to heat, they smoked but little, swelled much, and afforded a bulky coal.

4. Their solutions in water reddened litmus-paper.

5. The same solutions copiously precipitated the metallic salts, especially muriate of tin, acetite of lead, and oxysulphate of iron. The colour of these precipitates was commonly brown, inclining to that of chocolate, excepting the tin, which was blackish-gray.

6. They precipitated gold from its solution, in the metallic state.

7. They also precipitated the earthy salts, such as the nitrates of lime, barytes, &c. &c.

8. The fixed alkalis, as well as ammonia, when first added

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dissolved, have been noticed by Professor LICHTENSTEIN in CRELL's Chemical Annals, 1786; by Mr. LOWITZ; (CRELL's Chem. Journal, translated into English, Vol. II. p. 255;) and by Mr. JAMESON, in his Outline of the Mineralogy of the Shetland Islands, &c. 8vo. edit. p. 167.

to these solutions only deepened the colour, but, after some hours, rendered them turbid.

9. Glue or isinglass was immediately precipitated by these solutions from water, and the precipitates were more or less brown according to the strength of the solutions. The precipitates were also insoluble in cold and in boiling water, so that in their essential properties they proved similar to those formed by the varieties of tannin hitherto known, with the difference, that this factitious substance appeared to be exempt from gallic acid, and mucilage, which commonly accompany the varieties of tannin, and which occasion modifications in the colour and appearance of some of their precipitates.

Having thus had the satisfaction to discover that a product resembling tannin could be formed by such a simple method, not only from vegetable but also from mineral coal, I was induced to examine how far the same might be extended to animal coal, and I therefore reduced a portion of isinglass to that state in a close vessel, and having rubbed it into fine powder, I digested it with nitric acid in the manner which has been described. At first the acid did not appear to act upon it, but at length it was slowly dissolved excepting a small quantity, which however was in every respect unchanged; and here we may remark, that as animal coal is incinerated with much more difficulty than vegetable coal or charcoal, so was the same difference to be observed, when oxygen was presented to these bodies in the humid way.

The solution resembled those which have been described, excepting, that the brown colour was more intense. It was evaporated to dryness, and was then dissolved in distilled water, after which, the solution being examined by the re-agents which had been employed in the former experiments, was found to produce similar effects, excepting some difference in the colour of the precipitates.

I next added some of the liquid to a solution of isinglass, and obtained a copious precipitate. Thus it is evident, that a tanning substance may be formed from animal as well as from vegetable and mineral coal; and it is not a little curious, that this enables us to assert as a matter of fact, although not of economy, that one portion of the skin of an animal may be employed to convert the other into leather.

In the course of these experiments, I also subjected coak to the action of nitric acid, and obtained a product which resembled that which had been afforded by pit-coal; but in this case (as might be expected) there was not any appearance of the deep yellow substance which has so often been mentioned.

These experiments therefore prove, that a tanning substance may be artificially formed by exposing carbon to the action of nitric acid; and it also appears, that this is best effected when the carbon is uncombined with any other substance excepting oxygen. The following experiments seem to corroborate this opinion.

1. A piece of Bovey coal, which had perfectly the appearance of half-charred wood, was reduced to powder, and was digested with nitric acid until the whole was dissolved.

The colour of the solution was deep yellow; and, by evaporation, a yellow viscid mass was obtained, which was dissolved in distilled water. This solution was then examined by various re-agents, and particularly by gelatine, but not any vestige of tanning matter could be discovered, and the predominant substance appeared to be oxalic acid.

2. Another piece of Bovey coal, which had less of the characters of wood, and was more perfectly carbonized, was treated in the way which has been described; the solution was brown, and, unlike the former, afforded a considerable precipitate with gelatine.

3. A portion of the first sort of Bovey coal was exposed to a red heat in a close vessel, and was then reduced to powder and digested with nitric acid; here a remarkable difference was to be observed, for nearly the whole was thus converted into the tanning substance.

4. A coal from Sussex, extremely like the second sort of Bovey coal, also afforded the same product.

5. A piece of the Surturbrand from Iceland yielded a similar result.

6. Some deal saw-dust was digested with the nitric acid until it was completely dissolved; by evaporation a yellow viscid mass was obtained, the solution of which in water afforded results like those of the first experiment on the Bovey coal, for oxalic acid was found in it, but not any of the tanning substance.

7. Another portion of the same deal saw-dust was converted into charcoal in a close vessel; the charcoal was then treated in the manner already described, and was thereby formed into a liquid which copiously precipitated gelatine.

8. Having previously ascertained that teak wood does not contain gallic acid nor tannin, I reduced some of it into charcoal, which was afterwards readily converted into the substance above mentioned.

In these experiments, the deal and the teak wood had been reduced to the state of coal, as usual, by fire, but as this does not appear to have been the means generally employed by nature to convert organized substances into the varieties of mineral coal, I for a considerable time, previous to the discovery of the artificial tanning product, had been employed in a series of experiments on the slow carbonization of a great number of vegetable substances by the humid way.

The agent which I most commonly used to produce this effect, was sulphuric acid occasionally diluted; and although many of the processes were extremely unpleasant and tedious, yet I have not any reason to regret the time which has been thus employed. The subject however I foresee will branch out into several details, none of which as yet I can regard as sufficiently completed to merit the honour of being submitted to this learned Society; but I am in a manner almost compelled in the present case to anticipate a few of the experiments, with their results, because they are intimately connected with the subject now under consideration.

In these experiments, I have observed that concentrated sulphuric acid, when poured upon any of the resinous substances reduced to powder, dissolved them in a few minutes; at this period the solution was transparent, commonly of a yellowish-brown colour, and of the consistency of a viscid oil. But if, after this, the vessel was placed on a sand-bath, the colour of the solution became progressively darker, sulphureous gas was evolved, and at length the whole appeared like a very thick liquid of an intense black. I purposely for the present pass over many phenomena, some of which are peculiar to the different substances when thus treated, whilst others are general, and may be referred to those attendant on etherification, for my intention here is only to notice, in a concise manner, such as immediately tend to elucidate the subject of this Paper.

When concentrated sulphuric acid is poured on the common turpentine of the shops, it almost immediately dissolves it like the solid resins; and if a portion of this solution be poured into cold water, the turpentine is precipitated in the solid brittle state of common yellow resin. But if a second portion of the same solution, after the lapse of an hour or more, be in like manner poured into cold water, the resin thus formed is not yellow but dark brown; and if four or five hours are suffered to elapse before a third portion is poured into water, the resin is found to be completely black. After this, supposing the digestion to be carried on during several days, or until there is no longer any production of sulphureous gas, the turpentine or resin will be found converted into a black porous coal, which, if the operation has been properly conducted, does not contain any resin, although a substance may frequently be separated by digestion in alcohol, which I shall soon have occasion to notice.

When common resin was thus treated, I obtained about 43 *per cent.* of the coal, which, after being exposed to a red heat in a loosely covered platina crucible, still amounted to 30 *per cent.* and by the slowness of its combustion and other circumstances which need not here be related, approached very nearly to the characters of some of the mineral coals.*

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* The difference of the quantity of carbon, which may be obtained in the state of coal from resinous substances by the humid and by the dry way, is very considerable; we may take common resin as an example, for when 100 grains were exposed to simple

The effects produced by sulphuric acid upon turpentine and resin are manifestly caused by the union of the two constituent principles of the latter (namely, hydrogen and carbon) with part of the oxygen of the former, so that sulphureous acid, water, and coal are produced. I therefore availed myself of this process, by which coal could be progressively formed whilst the original substance was gradually decomposed, to make the following experiment.

A quantity of common turpentine was treated with sulphuric acid in the way which has been described, and different portions of the solution being poured at different periods into water whilst the remainder was digested during several days, I thus obtained from the same original substance, yellow resin, brown resin, black resin, and coal. I then digested a portion of each of these, as well as some of the turpentine, in separate vessels with nitric acid until they were completely dissolved, and afterwards reduced them to dryness. The different residua varied in colour from yellow to dark brown, corresponding to the substances which had been employed. These were then dissolved in distilled water, and were examined by solution of isinglass and other reagents.

1. The solution of the residuum of turpentine was pale straw colour, and did not precipitate gelatine.

2. That of yellow resin resembled the former in every respect.

3. That of the brown resin was of a deeper yellow, but in other particulars resembled the above.

4. That of the black resin on the contrary yielded a considerable portion of the tanning substance,—and

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distillation in a small glass retort placed over an open charcoal fire, the residuum of coal only amounted to $\frac{1}{4}$ of a grain.

5. That of the coal afforded it in great abundance.

Hence it appears, that these different modifications of turpentine yielded the tanning substance only in proportion to the quantity of their original carbon, which, by oxidizement, had been progressively converted into coal.*

Other substances, when reduced into coal in the humid way, were in like manner formed into the tanning substance by nitric acid. In fact I found this to be the constant result, and amongst the many substances which were examined, I shall mention various kinds of wood, copal, amber, and wax, all of which, when reduced to coal by sulphuric acid, yielded similar products, by subsequent treatment with nitric acid.

But this substance may likewise be artificially produced without the help of nitric acid, although in a less proportion, as well as with some slight variations in its characteristic properties; for, as I have already observed, when any of the resins or gum resins (such as common resin, elemi, asa fœtida, &c.) have been long digested with sulphuric acid so as to assume the appearance and general characters of coal, if afterwards they are digested with alcohol, a portion is dissolved, and a dark brown solution is formed which by evaporation yields a mass soluble in water as well as in alcohol, and which precipitates gelatine, acetite of lead, and muriate of tin, but produces only a very slight effect on oxysulphate of iron. This substance, therefore, which may thus be separated by alcohol from the coal formed from resinous bodies by sulphuric acid, evidently contains some of the tanning matter, which has been produced during the carbonization of those substances.

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* Some late experiments have however convinced me that carbon need not be absolutely converted into coal in order to produce the artificial tanning substance; but this will be more fully explained in a subsequent Paper.

A natural process very similar to this, I much suspect takes place in some cases where peat is formed; I say in some cases, because the production of tanning matter does not seem to be absolutely a necessary consequence attendant on the formation of peat; for in many places where the latter abounds, the former cannot be detected, whilst in others, it is very abundant, and acts powerfully on animal bodies which have accidentally been exposed to its effects.

There are many facts of this kind upon record, such as the account of the bodies of the man and woman preserved in the moors near the woodlands in Derbyshire, and also of the woman found in the morass at Axholm, in Lincolnshire.* Now I am much inclined to believe, that the tanning substance which so much abounds in these and some other peat moors, did not originally exist in the vegetable substances from which the peat has been produced, but that it has been and continues to be progressively formed (under certain favourable circumstances) during the gradual carbonization and conversion of the vegetable matter into peat.

§ III.

In most of the former papers which I have had the honour to lay before the Royal Society, I have for greater perspicuity generally concluded with a recapitulation of the contents, but in the present case, this appears to be superfluous, as the whole may be concentrated into one simple fact, namely, that a substance very analogous to tannin, which has hitherto been considered as one of the proximate principles of vegetables, may at any time be produced, by exposing carbonaceous substances,

* Phil. Trans. Vol. XXXVIII. p. 413. Ibid. Vol. XLIV. p. 571.

whether vegetable, animal, or mineral, to the action of nitric acid.

Since the preceding experiments were made, I have farther proved the efficacy of this substance by actual practice, and have converted skin into leather by means of materials which, to professional men, must appear extraordinary, such as, deal saw-dust, asphaltum, common turpentine, pit-coal, wax candle, and a piece of the same sort of skin.

Allowing, therefore, that the production of this substance must for the present be principally regarded only as a curious chemical fact not altogether unimportant, yet as the principle on which it is founded appears to be developed, we may hope, that a more economical process will be discovered, so that every tanner may be enabled to prepare his leather even from the refuse of his present materials.

The organized bodies and their products have only of late years much attracted the attention of chemists, many of whom, even at this time, (although the modes of chemical examination have been so much improved,) seem disgusted and deterred by the Proteus-like changes which take place whenever these substances are subjected to experiment.

But these variable and endless alterations of their properties seem rather calculated to operate as incitements to investigation; for by the accumulation of facts resulting from the changes produced in these bodies by disuniting and by recombining their elementary principles, not only will chemistry as a science become farther illumined and extended, but it will, as it has hitherto done, render great and essential services to the arts and manufactures.

XIII. *The Case of a full grown Woman in whom the Ovaria were deficient. By Mr.* Charles Pears, *F. L. S. Communicated by the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read May 9, 1805.

THE following case is laid before this learned Society, as an addition to those already registered in the Philosophical Transactions, with the view of elucidating such physiological inquiries as are connected with the state of the organs of generation.

ANN JOSEPH was born at Diserth, in Radnorshire, North Wales, August 4, 1770. She was of a fair florid complexion, and blue eyes, dark-brown hair, a flat nose, and thick lips. She was naturally mild, but when irritated, was sometimes malicious and revengeful. In her diet she was remarkably abstemious, eating little of animal food, no fresh vegetables, and so small a portion of bread, that she often did not consume a penny loaf in the course of a week. If at any time she was prevailed upon to take several kinds of food, her stomach was so much affected by it, that she fainted away; and if she had eaten a hearty meal, these faintings would be repeated.

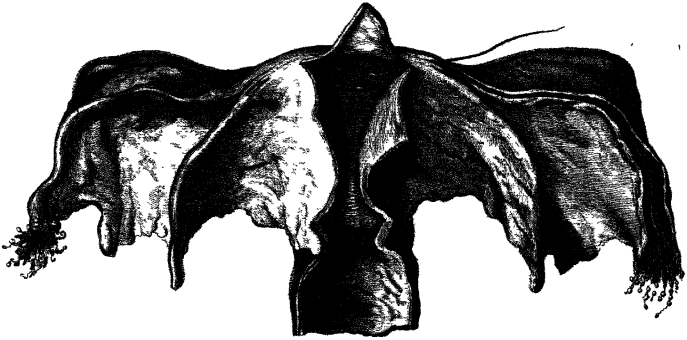
She was of a costive habit, seldom having a passage in her bowels oftener than once in nine days, and sometimes only once in fourteen. She slept well, and could endure hard work, but was slow in performing it. Having ceased to grow at ten years of age, she was in stature not more than 4 feet 6 inches high. Her breadth across the shoulders was as much as 14 inches, but her *pelvis*, (contrary to what is usually observed in the proportions of the female skeleton,) measured only 9 inches, from the *ossa ilia* across the *sacrum*. Her breasts and nipples never enlarged more than in the male subject; she never menstruated: there was no appearance of hair on the pubes, nor were there any indications of puberty either in mind or body, even at twenty-nine years of age; on the contrary, she always expressed aversion to young men who were too familiar with her.

At the age of twenty-one she expressed much uneasiness at finding herself different from other young women, which she attributed to not having menstruated; and was so desirous of relief that she frequently took medical advice for that purpose.

From her infancy also she was liable to complaints in her chest, attended with cough, that came upon her at intervals in violent attacks, and increased in violence as she advanced in life. In her twenty-ninth year one of these attacks came on, attended with convulsions, of which she died after a few hours illness.

After death, the female organs were taken out and preserved. In this state they were shown to Sir JOSEPH BANKS, at whose request their internal structure was accurately examined, and the following appearances were observed, which are illustrated in the annexed drawing of them, made by Mr. CLIFT.

The os tincæ and uterus had the usual form, but had never increased beyond their size in the infant state; the passage into the uterus through the cervix was oblique. The cavity of



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the uterus was of the common shape, and the Fallopian tubes were pervious to the fimbriæ; the coats of the uterus were membranous.

The ovaria were so indistinct as rather to show the rudiments which ought to have formed them, than any part of their natural structure. All these appearances will be better understood by their representation in the annexed drawing (Plate V.) than from any description that can be given.

The history of this case, with the account of the dissection, becomes valuable, as it shows that an imperfect state of the ovaria is not only attended with an absence of all the characters belonging to the female after puberty, but that the uterus itself, although perfectly formed, is checked in its growth for want of due structure of those parts.

That there is an intimate connection between the ovaria and uterus has long been ascertained; but that the growth of the uterus should so entirely depend upon that of the ovaria, I believe to be a new fact; at least it has not been published in any work that has come under my observation.

XIV. *A Description of Malformation in the Heart of an Infant. By Mr.* Hugh Chudleigh Standert. *Communicated by* Anthony Carlisle, *Esq. F. R. S.*

Read May 9, 1805.

The child, from whom the subject of the present description was taken, died at the age of ten days; during which period all the animal functions seemed to have been regularly discharged, with this exception, that the skin exhibited the purple or blue colour, so often noticed in cases of imperfect pulmonary circulation.

The body was fleshy, somewhat less than the usual size, and the extremities were livid. All the viscera were in a natural state, except the heart, which presented the following remarkable structure.

On viewing it externally, only one auricle could be observed, into which the pulmonary veins, and venæ cavæ, entered in their ordinary directions. The pulmonary artery was wholly deficient; and, on dissection, it appeared that the body of the heart possessed but one ventricle, separated from the auricle by tendinous valves, and opening into the aorta.

The auricle was also single, having a narrow muscular band, which crossed the ostium venosum, in the place of the septum. The aorta sent off an artery, from the situation of the ductus arteriosus, which divided itself into two branches, supplying each mass of the lungs. These vessels were of small diameter.

The arterial system had been injected with wax, and, in removing the heart from the thorax, this pulmonary branch of the aorta was unfortunately cut away.

The pulmonary veins were four in number; but neither the area of these veins, nor that of the vessel which acted as the pulmonary artery, exceeded half the common dimensions.

This child, when alive, came under the observation of Dr. COMBE, who did not perceive that its respiration, temperature, or muscular action, were materially affected. In the records of malformation of the heart, the present case is extraordinary, resembling in organization the amphibious animals, rather than the mammalia. That an infant should have existed so long, under such circumstances, carrying on all the vital functions, appears a physiological fact of some importance, especially as the dependence of life on respiration, and the changes produced in the vascular system, are so imperfectly understood.

EXPLANATION OF THE DRAWING. (Plate VI.)

Fig. 1. A view of the left side of the heart, the common ventricle being opened by a simple incision, showing the valves of the ostium.

a, The aorta.

b, The common trunk of the two branches of the right pulmonary veins.

c, The vena cava superior.

d, *e*, The two trunks of the left pulmonary veins.

Fig. 2. A view of the right side of the heart, exhibiting the common cavity of the auricle, a portion of its parietes being cut away, with the vena cava inferior.

a, *c*, The two trunks of the left pulmonary vein.

b, The vena cava superior.

d, The aorta.

e, The trunk of the right pulmonary veins.

f, The muscular band in the auricle.

These drawings are of the natural size, and the subject of them is preserved in the collection of DR. COMBE.

Fig. 1.

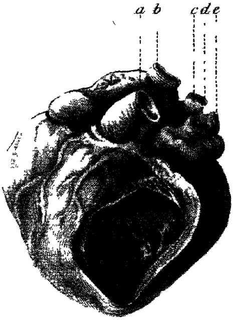
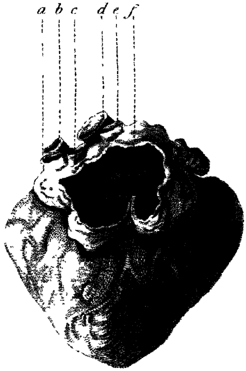


Fig. 2



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XV. *On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By* Humphry Davy, *Esq. F.R.S. Professor of Chemistry in the Royal Institution.*

Read May 16, 1805.

I HAVE found the boracic acid a very useful substance for bringing the constituent parts of stones containing a fixed alkali into solution.

Its attraction for the different simple earths is considerable at the heat of ignition, but the compounds that it forms with them are easily decomposed by the mineral acids dissolved in water, and it is on this circumstance that the method of analysis is founded.

The processes are very simple.

100 grains of the stone to be examined in very fine powder, must be fused for about half an hour, at a strong red heat, in a crucible of platina or silver, with 200 grains of boracic acid.

An ounce and half of nitric acid, diluted with seven or eight times its quantity of water, must be digested upon the fused mass till the whole is decomposed.

The fluid must be evaporated till its quantity is reduced to an ounce and half or two ounces.

If the stone contain silex, this earth will be separated in the process of solution and evaporation; and it must be collected upon a filter, and washed with distilled water till the boracic acid and all the saline matter is separated from it.

The fluid, mixed with the water that has passed through the filter, must be evaporated, till it is reduced to a convenient quantity, such as that of half a pint; when it must be saturated with carbonate of ammonia, and boiled with an excess of this salt, till all the materials that it contains, capable of being precipitated, have fallen to the bottom of the vessel.

The solution must then be separated by the filter, and the earths and metallic oxides retained.

It must be mixed with nitric acid till it tastes strongly sour, and evaporated till the boracic acid appears free.

The fluid must be passed through the filter, and subjected to evaporation till it becomes dry; when, by exposure to a heat equal to 450° FAHRENHEIT, the nitrate of ammonia will be decomposed, and the nitrate of potash or soda will remain in the vessel.

It will be unnecessary for me to describe minutely the method of obtaining the remaining earths and metallic oxides free from each other, as I have used the common processes. I have separated the alumine by solution of potash, the lime by sulphuric acid, the oxide of iron by succinate of ammonia, the manganese by hydrosulphuret of potash, and the magnesia by pure soda.

XVI. *On the Direction and Velocity of the Motion of the Sun, and Solar System. By* William Herschel, *LL. D. F. R. S.*

Read May 16, 1805.

Our attention has lately been directed again to the construction of the heavens, on which I have already delivered several detached papers. The changes which have taken place in the relative position of double stars, have ascertained motions in many of them, which are probably of the same nature with those that have hitherto been called proper motions. It is well known that many of the principal stars have been found to have changed their situation, and we have lately had a most valuable acquisition in Dr. Maskelyne's Table of proper motions of six and thirty of them. If this Table affords us a proof of the motion of the stars of the first brightness, such as are probably in our immediate neighbourhood, the changes of the position of minute double stars that I have ascertained, many of which can only be seen by the best telescopes, likewise prove that motions are equally carried on in the remotest parts of space which hitherto we have been able to penetrate.

The proper motions of the stars have long engaged the attention of astronomers, and in the year 1783, I deduced from them, with a high degree of probability, a motion of the sun and solar system towards λ Herculis. The reasons which were then pointed out for introducing a solar motion, will now be much strengthened by additional considerations; and the above mentioned Table of well ascertained proper motions will also enable us to enter rigorously into the necessary calculations for ascertaining its direction, and discovering its velocity. When these points are established, we shall be prepared to draw some consequences from them that will account for many phenomena which otherwise cannot be explained.

The scope of this Paper, wherein it is intended to assign not only the direction, but also the velocity of the solar motion, embraces an extensive field of observation and calculation; but as to give the whole of it would exceed the compass of the present sheets, I shall reserve the velocity of the solar motion for an early future opportunity, and proceed now to a disquisition of the first part of my subject, which is the direction of the motion of the sun and solar system.

Reasons for admitting a solar Motion.

It may appear singular that, after having already long ago pointed out a solar motion, and even fixed upon a star towards which I supposed it to be directed, I should again think it necessary to show that we have many substantial reasons for admitting such a motion at all. What has induced me to enter into this inquiry is, that some of the consequences hereafter to be drawn from a solar motion when established, seem to condradict the very intention for which it is to be introduced. The chief object in view, when a solar motion was proposed to be deduced from observations of the proper motions of stars, was to take away many of these motions by investing the sun with a contrary one. But the solar motion, when its existence has been proved, will reveal so many concealed real motions, that we shall have a greater sum of them than it would be necessary to admit, if the sun were at rest; and, to remove this objection, the necessity for admitting its motion ought to be well established.

Theoretical Considerations.

A view of the motion of the moons, or secondary planets, round their primary ones, and of these again round the sun, may suggest the idea of an additional motion of the latter round some other unknown center; and those who like to indulge in fanciful reviews of the heavens, might easily build a system upon hypotheses not altogether without some plausibility in their favour. Accordingly we find that Mr. LAMBERT, in a work which is full of the most fantastic imaginations, has framed a system wherein the sun is supposed to move about the nebula in Orion.* But, setting aside the extravagant idea of making this luminous spot a center of motion, it must certainly be admitted that the solar motion itself is at least a very possible event.

I have already mentioned, in a note to my former Paper,† that the possibility of a solar motion has also been shown from theoretical principles by the late Dr. WILSON of Glasgow; and its probability afterwards, from reasons of the same nature, by Mr. DE LA LANDE. The rotatory motion of the sun, from which he concludes a displacing of the solar center, must certainly be allowed to indicate a motion of translation in space; for though it may be possible, it does not appear probable, that any mechanical impression should have given the former, without occasioning the latter. But, as we are intirely unacquainted with the cause of the rotatory motion, the solar translation in space from theoretical reasons, can only be admitted as a very plausible hypothesis.

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* See *Système du Monde de* Mr. LAMBERT, page 152, and 158.

† See Phil. Trans. for the year 1783, page 283.

It would be worth while for those who have fixed instruments, to strengthen this argument by observing the stars which are known to change their magnitudes periodically. For as we have great reason to ascribe these regular changes to a rotatory motion of the stars,* a real motion in space may be expected to attend it; and the number of these stars is so considerable that their concurring testimony would be very desirable.

Perhaps Algol, which according to these ideas must have a very quick rotatory motion, may be found to have also a considerable progressive one; and if that should be ascertained, the position of the axis of the rotation of this star will be in a great measure thereby discovered.

An argument from the real motion to a rotatory one is nearly of equal validity, and therefore all the stars that have a motion in space may be surmised to have also a rotation on their axes.

Symptoms of parallactic Motions.

But, setting aside theoretical arguments, I shall now proceed to such as may be drawn from observation; and, as all parallactic motions are evident indications that the observer of them is not at rest, it will be necessary to explain three sorts of motions, of which the parallactic is one; they will often engage our attention in the following discussion.

Let the sun be supposed to move towards a certain part of the heavens, and since the whole solar system will have the same motion, the stars must appear to an inhabitant of the earth to move in an opposite direction. In the triangle *s p a*, Plate VII. Fig. 1, let *s p* represent the parallactic motion of a star; then, if this star is one that has no real motion, *sp* will also be its apparent motion; but if the star in the same time, that by its parallactic motion it would have gone from *s* to *p*, should have a real motion which would have carried it from *s* to *r*, then will it be seen to move along the diagonal *s a*, of the parallelogram *s r p a;* and *p a*, which is parallel and equal to *s r*, will represent its real motion. Therefore, in the above mentioned triangle *s p a*, which I suppose to be formed in the concave part of the heavens by three arches of great circles, the eye of the observer being in the center, the three sides will represent, or stand for, the three motions I have named: *sp* the parallactic, *pa* the real, and *sa* the apparent motion of the star. The situation and length of these arches, in seconds of a degree, will express, or rather represent, not only the direction but also the quantity of each motion, such as it must appear to an eye in the above mentioned central situation. And calling the solar motion S, the distance of the star from the sun *d*, and the sine of the star's distance from the point towards which the sun is moving φ, the parallactic motion, when these are given, will be had by the expression $\frac{\varphi . S}{r . d} = sp$. This theorem, and its corollaries, of which frequent use will be made hereafter, it will not be necessary here to demonstrate.

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* See Phil. Trans. for the year 1795, page 68.

When I call the arch *pa* the real motion, it should be understood that I only mean its representative; for it must be evident that the absolute motion of a star in space, as well as its intrinsic velocity, will still remain unknown, because the inclination of that motion on which also its real velocity will depend, admits of the greatest variety of directions. We are only acquainted with the plane in which the motion must be performed, and with the length of the arch in seconds by which that motion may be measured. We may add that the chords of the arches representing the three motions are the smallest velocities of these motions that can be admitted; for in every other direction but at right angles to the line of sight, the actual space over which the star will move must be greater than the arch or chord by which its motion is represented.

Now, since a motion of the sun will occasion parallactic motions of the stars, it follows that these again must indicate a solar motion; but in order to ascertain whether parallactic motions exist, we ought to examine those stars which are most liable to be visibly affected by solar motion. This requisite points out the brightest stars as the most proper for our purpose; for any star may have a great real motion, but in order to have a great parallactic one, it must be in the neighbourhood of the sun. And as we can only judge of the distance of the stars by their splendour we ought to choose the brightest, on account of a probability that, being nearer than faint ones, they may be more within the reach of parallax, and thus better qualified to show its effects.

We are also to look out for a criterion whereby parallactic may be distinguished from real motions; and this we find in their directions. For if a solar motion exists, all parallactic motions will tend to a point in opposition to the direction of that motion; whereas real motions will be dispersed indiscriminately to all parts of space.

With these distinctions in view, we may examine the proper motions of the principal stars; for these, if the sun is not at rest, must either be intirely parallactic, or at least composed of real and parallactic motions; in the latter case they will fall under the denomination of one of the three motions we have defined, namely *sa*, the apparent motion of the star.

In consequence of this principle I have delineated the meeting of the arches arising from a calculation of the proper motions of the 36 stars in Dr. MASKELYNE's catalogue, on a celestial globe; and, as all great circles of a sphere intersect each other in two opposite points, it will be necessary to distinguish them both: for, if the sun moves to one of them, it may be called the apex of its motion, and as the stars will then have a parallactic motion to the opposite one, the appellation of a parallactic center may very properly be given to it. The latter falling into the southern hemisphere, among constellations not visible to us, I shall only mention their opposite intersections; and of these I find no less than ten that are made by stars of the first magnitude, in a very limited part of the heavens, about the constellation of Hercules. Upon all the remaining surface of the same globe there is not the least appearance of any other than a promiscuous situation of intersections; and of these only a single one is made by arches of principal stars.

The ten intersecting points made by the brightest stars are as follows. The 1st is by Sirius and Arcturus, in the mouth of the Dragon. The 2d by Sirius and Capella, near the following hand of Hercules. The 3d by Sirius and Lyra, between the hand and knee of Hercules. The 4th by Sirius and Aldebaran, in the following leg of Hercules. The 5th by Arcturus and Capella, north of the preceding wing of the Swan. The 6th by Arcturus and Aldebaran, in the neck of the Dragon. The 7th by Arcturus and Procyon, in the preceding foot of Hercules.

The 8th by Capella and Procyon, south of the following hand of Hercules. The 9th by Lyra and Procyon, preceding the following shoulder of Hercules. And the 10th is made by Aldebaran and Procyon, in the breast of Hercules.

The following Table gives the calculated situation of these ten intersections in right ascension and north polar distance.

Table I.

No.	Right Ascension.			Polar Distance.		
1	255°	39′	50″	36°	41′	34″
2	275	9	32	64	21	48
3	272	23	58	58	23	24
4	263	25	38	44	39	47
5	290	0	58	32	7	23
6	267	2	19	33	57	20
7	235	3	13	46	21	34
8	272	51	49	73	7	56
9	266	46	49	66	48	11
10	260	1	29	60	59	34

We might rest satisfied with having shown that the parallactic effect of which we are in search is plainly to be perceived in the motion of the brightest stars; however, by way of further confirmation, we may take in some large stars of the next order, in whose motions evident marks of the influence of parallax may likewise be perceived. When the intersections made by their proper motions and the arches in which the stars of the first magnitude are moving, are examined, we find no less than fifteen which unite with the former ten, in pointing out the same part of the heavens as a parallactic center. It will be sufficient only to mention the opposite points of the situation of these intersections, and the stars by which they are made, without giving a calculated table of them.

The 1st is the following leg of Hercules, and is made by Sirius and β Tauri. The 2d is also in the following leg of Hercules, by Sirius and α Andromedæ. The 3d is in the following hand of Hercules, by Sirius and α Arietis. The 4th in the neck of the Dragon, by Arcturus and β Tauri. The 5th between the Lyre and the northern wing of the Swan, by Capella and α Andromedæ. The 6th near the following hand of Hercules, by Capella and α Arietis. The 7th preceding the head of Hercules, by Lyra and β Tauri. The 8th between the Lyre and northern wing of the Swan, by Lyra and α Andromedæ. The 9th in the following arm of Hercules, by Lyra and α Arietis. The 10th in the following leg of Hercules, by Aldebaran and β Tauri. The 11th in the following leg of Hercules, by Aldebaran and α Andromedæ. The 12th in the head of Hercules, by Aldebaran and α Arietis. The 13th in the following arm of Hercules, by Procyon and β Tauri. The 14th in the back of Hercules, by Procyon and α Andromedæ. And the 15th near the following arm of Hercules, is made by Procyon and α Arietis.

An argument like this, founded upon the most authentic observations, and supported by the strictest calculations, can hardly fail of being convincing. And though only the ten principal apices of the twenty-five that are given have been calculated, the other fifteen may nevertheless be depended upon as true to less than one degree of the sphere.

Changes in the Position of double Stars.

We have lately seen that the alterations in the relative situation of a great number of double stars may be accounted for by a parallactic motion. Among the 56 stars which I have given, the changes of more than half of them appear to be of this nature; and it will certainly be more eligible to ascribe them to the effect of parallax than to admit so many separate motions in the different stars; especially when it is considered that if the alterations of the angle of position were owing to a motion of the largest star of each set, the direction of such motions must, in contradiction to all probability, tend nearly to one particular part of the heavens.

This argument, drawn from the change of the position of double stars, may be considered as deriving its validity from the same source with the former, namely, the parallactic motions of at least 28 more stars, pointing out the same apex of a solar motion by their direction to its opposite parallactic center.

Incongruity of proper Motions.

It may be remarked that the proper motions of the stars, if they were in reality such as they appear to be, would contain a certain incongruous mixture of great velocity and extreme slowness. Arcturus alone describes annually an arch of more than two seconds: Aldebaran hardly one-tenth and a quarter of a second: Rigel little more than one-tenth and a half: even Lyra moves barely three and a quarter tenths of a second, while Procyon has almost four times that velocity. Out of 36 stars whose proper motion we have examined, there are 15 that do not reach two-tenths of a second: β Virginis moves seventy-seven hundredths, and α Cygni only six. But it will be shown, when the direction and velocity of the solar motion come to be explained, that these kind of incongruities are mere parallactic appearances; and that there is so general a consistency among the real motions of the stars, that Arcturus is in no respect singled out as a star whose motion is far beyond the rest.

By giving this remark a place among the reasons for admitting a solar motion, it is not intended to lay any particular stress upon it; for it may be objected that our idea of the congruence or harmony of the celestial motions can be no criterion of their real fitness and symmetry. But when such discordant proper motions as those I have mentioned in stars of no very different lustre are under consideration, and may be easily shown to be only parallactic phenomena, the method by which this can be done must certainly appear eligible, and when added to many other inducements, will throw some share of weight into the scale.

Sidereal Occultation of a small Star.

Of nearly the same importance with the former argument is the account of the occultation of a small star by a large one, which I have given in my last Paper. When the solar motion has been established, we shall prove that the vanishing of the small star near δ Cygni, as far as we can judge at present, is only a parallactic disappearance. It must be granted that a real motion of the large star would also explain the same phenomenon; but then again, this star must be supposed to move towards the very same parallactic center which the changes in the position of other double stars point out, and this cannot be probable.

Direction of the solar Motion.

From what has been said, I believe the expedience of admitting a solar motion will not be called in question; our next endeavour therefore must be to investigate its direction.

To return to the before mentioned intersections of the arches, in which the proper motions of the stars are performed, I shall begin by proving that when the proper motions of two stars are given, an apex may be found, to which, if the sun be supposed to move with a certain velocity, the two given motions may then be resolved into apparent changes, arising from sidereal parallax, the stars remaining perfectly at rest.

Let the stars be Arcturus and Sirius, and their annual proper motions as given in the Astronomer Royal's Tables.

When the annual proper motion of Arcturus, which is $-1'',26$ in right ascension, and $+1'',72$ in north polar distance, is reduced by a composition of motions to a single one, it will be in a direction which makes an angle of 55° 29′ 42″ south-preceding with the parallel of Arcturus, and of a velocity so as to describe annually 2″,08718 of a great circle.

The annual proper motion of Sirius, $-0'',42$ in right ascension, and $+1'',04$ in north polar distance, by the same method of composition, becomes a motion of 1″,11528, in a direction which makes an angle of 68° 49′ 41″ south-preceding with the parallel of Sirius.

By calculation, the arches in which these two stars move, when continued, will meet in what I have called their parallactic center, whose right ascension is 75° 39′ 50″, and south polar distance is 36° 41′ 34″. The opposite of this, or right ascension 255° 39′ 50″, and north polar distance 36° 41′ 34″, is what we are to assume for the required apex of the solar motion.

When a star is situated at a certain distance from the sun, which we shall call 1; and 90° from the apex of the solar motion, its parallactic motion will be a maximum. Let us now suppose the velocity of the sun to be such that its motion, to a person situated on this star, would appear to describe annually an arch of 2″,84825, or, which is the same thing, that the star would appear to us, from the effect of parallax, to move over the above mentioned arch in the same time.

To apply this to Arcturus, we find by calculation that its distance from the apex of the solar motion is 47° 7′ 6″; its parallactic motion therefore, which is as the sine of that distance, will be 2″,08718; and this, as has been shown, is the apparent motion which observation has established as the proper motion of Arcturus.

In the next place, if we admit Sirius to be a very large star situated at the distance 1,6809 from us, and compute its elongation from the apex of the solar motion, we shall find it 138° 50′ 14″,5. With these two data we calculate that its parallactic motion will be $\frac{\varphi \,.\, S}{r \,.\, d} = sp = 1'',11528$; and this also agrees with the apparent motion which has been ascertained by observation as the proper motion of Sirius.

Now since, according to the rules of philosophising, we ought not to admit more motions than will account for the observed changes in the situation of the stars, it would be wrong to have recourse to the motions of Arcturus and Sirius, when that of the sun alone will account for them both; and this consideration would be a sufficient inducement for us to fix at once on the calculated apex, as well as on the relative distances that have been assigned to these stars, if other proper motions could with equal facility be resolved into similar parallactic appearances. But from the nature of proper motions, it follows, that when a third star does not lead us to the same apex as the other two, its apparent motion cannot be resolved by the effect of parallax alone. And to enhance our difficulties, the number of apices, that would be required to solve all proper motions into parallactic ones, increases not as the number of stars admitted to have proper motions, but, when their situation happens to be favourable, as the sum of an arithmetical series of natural numbers, beginning at 0, continued to as many terms as there are stars admitted: so that if two stars give only one apex, one star added to it will give three apices; and ten, for instance, will give no less than 45, and so on.

The method of reasoning which, on this subject, I have adopted, is so closely connected with astronomical observations that I shall keep them constantly in view; and therefore shall illustrate what has been advanced, by taking in Capella as a third star. The three apices which then are pointed out will be that in the mouth of the Dragon, by Arcturus and Sirius; a second under the northern wing of Cygnus, by Arcturus and Capella; and a third in the following hand of Hercules, by Sirius and Capella. The calculation of them is in Table I.

The annual proper motions of our third star in Dr. MASKELYNE's Tables are + 0″,21 in right ascension, and + 0″,44 in north polar distance; and by calculation these quantities give an annual motion of 0″,46374 to Capella, in a direction which makes an angle of 71° 35′ 22″,4 south-following with the parallel of this star.

The distance of Capella from the same calculated apex of the solar motion, by which we have already explained the apparent motions of the other two stars, is 80° 54′ 46″; and, admitting again the velocity of the sun towards the same point as stated before, it will occasion a parallactic motion of Capella, in a direction 89° 54′ 48″ south-following its parallel, amounting to 2″,8125. In this calculation Capella has been taken for a star of the first magnitude, supposing its distance from us to be equal to that of Arcturus.

By constructing then a triangle, the three sides of which will represent the three motions which every star must have that is not at rest in space; we have one of the sides, representing the apparent motion of the star, equal to 0″,4637; the other side, being the parallactic motion of the star 2″,8125; and the included angle 18° 19′ 27″. From these data we obtain the third side, representing the real motion of the star, which will be 2″,3757. By the given situation of this triangle with respect to the parallel of declination of Capella, the angle of the real motion will also be had, which is 86° 34′ 11″ north-following the parallel of this star. A composition of the parallactic and the real motion in the directions we have assigned, will produce the annual apparent motion which has been established by observation.

But to apply what has been said to our present purpose, it may be observed, that although we have accounted for the proper motion of our third star by retaining the same apex of the solar motion, which has given us an explanation of the apparent motions of the other two, yet in doing this we have been obliged to assign a great degree of real motion to Capella; and to this it may be objected, that we can have no authority to deprive Arcturus and Sirius of real motions, in order to give one of the same nature to our third star: and indeed to every star that has a proper motion which does not tend to the same parallactic center as the motions of Arcturus and Sirius.

This objection is perfectly well founded, and I have given the above calculation on purpose to show that, when we are in search of an apex for the solar motion, it ought to be so fixed upon as to be equally favourable to every star which is proper for directing our choice. Hence a problem will arise, in our present case, how to find a point whose situation among three given apices shall be so that, if the sun's motion be directed towards it, there may be taken away the greatest quantity of proper motion possible from the given three stars. The intricacy of the problem is greater than at first it may appear, because by a change of the distance of the apex from any one of the stars, its parallactic motion, which is as the sine of that distance, will be affected; so that it is not the mere alteration of the angle of direction, which is concerned. However, it will not be necessary to enter into a solution of the problem; for it must be very evident that a much more complex one would immediately succeed it, since three stars would certainly not be sufficient to direct us in our present endeavour to find the best situation of an apex for the solar motion; I shall therefore now leave these stars, and the apices pointed out by them, in order to proceed to a more general view of the subject.

We have already seen that the brightest stars are most proper for showing the effect of parallax, and that in our search after the direction of the solar motion, our aim must be to reduce the proper motions of the stars to their lowest quantities. The six principal stars, whose intersecting arches have been given, when their proper motions in right ascension and polar distance are brought into one direction, will have the apparent motions contained in the following Table.

Table II.

Names of the Stars.	Direction of the apparent Motions.	Quantities of the apparent Motions.
Sirius -	68° 49′ 40″,7 south-preceding	1″,11528 per year
Arcturus -	55 29 42,0 south-preceding	2,08718 ———
Capella -	71 35 22,4 south-following	0,46374 ———
Lyra - -	56 20 57,3 north-following	0,32435 ———
Aldebaran	76 29 37,3 south-following	0,12341 ———
Procyon -	50 2 24,5 south-preceding	1,23941 ———
	Sum of the apparent motions	5″,35337

We must now recur to what has been said, when the construction of the triangle expressing the three motions of a star, that is not at rest, was explained; and, as we are to find out a solar motion which will require the least real motion in our six stars, an attention to this triangle will be of considerable use; for when the line *pa*, Fig. 1, which represents the real motion, is brought into the situation *ma*, where it is perpendicular to *sp*, the real motion which is required will then be a minimum. It also follows, from the construction of the same triangle, that if by the choice of an apex for the solar motion we can lessen the angle made at *s* by the lines *sp* and *sa*, we shall lessen the quantity of real motion required to bring the star from the parallactic line *spm* to the observed position *a*.

It has already been shown, in the case of Sirius and Arcturus, that when two stars only are given, the line *sp* may be made to coincide with the lines *sa*, of both the stars, whereby their real motions will be reduced to nothing. It has also been proved, by adding Capella to the former two, that when three stars are concerned, some real motion must be admitted in one of them. Now, since all parallactic motions are directed to the same center, a single line may represent the direction of the effect of the parallax, not only of these three stars but of every star in the heavens. According to this theory, let the line *s*P or *s*S, in Fig. 2, stand for the direction of the parallactic motion of the stars; and as in the foregoing Table we have the angles of the apparent motion of six stars with the parallel of each star, we must now also compute the direction of the line *s*P or *s*S with the parallels of the same stars. This may be done as soon as an apex for the solar motion is fixed upon. The difference between these angles and the former will give the several parallactic angles P*sa* or S*sa*, required for an investigation of the least quantity *ma*, belonging to every star.

For instance, let the point towards which we may suppose the sun to move, be λ Herculis; and calculating the required angles of the direction in which the effect of parallax will be exerted, with the six stars we have selected for the purpose of our investigation, we find them as in the following Table.

Table III.

Angles of the parallactic Motion with the Parallel.

Sirius	32°	54′	8″,5	south-preceding.
Arcturus	17	23	45,7	south-preceding.
Capella	85	10	3,9	south-following.
Lyra	35	59	49,5	north-following.
Aldebaran	71	21	35,4	south-following.
Procyon	47	43	44,6	south-preceding.

The difference between these parallactic, and the former apparent angles, with the parallel of each star, will give the required angles for our second figure. They will be as follows.

Table IV.

Angles of the apparent with the parallactic Motion.

Sirius	35°	55′	32″,2	south-following.
Arcturus	38	5	56,3	south-following.
Capella	13	34	41,5	south-following.
Lyra	20	21	7,8	north-preceding.
Aldebaran	5	8	1,9	south-preceding.
Procyon	2	18	39,9	south-following.

By these angles, with the assistance of the lines *sa*, whose lengths represent the annual quantity of the apparent motions as given in our former Table, the Figure No. 2 has been constructed. When the situation of these angles is regulated as in that figure, we may draw the several lines *ma* perpendicular to SP, and, by computation, their value and sum will be obtained as follows.

Table V.

Quantities and Sum of the least real Motions.

	″
Sirius - - -	0,65437
Arcturus - - -	1,28784
Capella - - -	0,10887
Lyra - - -	0,11281
Aldebaran - - -	0,01104
Procyon - - -	0,04998
Sum	2″,22491.

The result of this investigation is, that by admitting a motion of the sun towards λ Herculis, the annual proper motions of our six stars, of which the sum is 5″,3537, may be reduced to real motions of no more than 2″,2249.

When first I proposed λ Herculis as an apex for the solar motion, it may be remembered that a reference to future observations was made for obtaining greater accuracy.* Such observations we have now before us, in the valuable Tables from which I have taken the proper motions of the six stars; and I shall prove that, with their assistance, we may fix on a solar motion that will be considerably more favourable.

We have already shown, that to ascertain the precise place of the best apex is attended with some difficulty; but from the inspection of the figure which represents the quantities of real motion required when λ Herculis is fixed upon, it will be seen that, by a regular method of approximation, we may turn the line SP into a situation where all the angles of the apparent motion of the six stars, will be much reduced. The quantities which are required for constructing another figure to represent the threefold motions of our six stars, when a different apex is fixed upon, are to be found by the same method we have pursued in the instance of λ Herculis; and the figure that has been given with respect to that star, shows evidently that the parallactic line SP should be turned more towards the line *sa*, representing the apparent motion of Sirius. We shall accordingly try a point near the following knee of Hercules, whose right ascension is 270° 15′, and north polar distance 54° 45′.

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* See Phil. Trans. for 1783, p. 273, line 8; and page 274, line 4.

The result of a calculation of the angles and the least quantities of real motion of our six stars, according to this apex, is collected in the following Table, and represented in Fig. 3.

Table VI.

Stars.	Angles of the parallactic Motion with the Parallel.	Angles of the apparent with the parallactic Motion.	Least Quantities of the real Motion.
Sirius -	68° 51′ 5″ south-preceding	0° 1′ 25″ south preceding	0″,0004561
Arcturus	29 30 32 south-preceding	25 59 10 south-following	0,9145072
Capella -	77 54 0 south-following	6 18 38 south-following	0,0509727
Lyra -	27 38 47 north-following	28 42 9 north-preceding	0,1557761
Aldebaran	66 20 17 south-following	10 9 21 south preceding	0,0217607
Procyon -	04 48 27 south-following	14 46 1 south-preceding	0,3159051
		Sum	1″,4593779

By this Table it appears that the annual proper motion of our six stars may be reduced to 1″,4594, which is 0″,7655 less than the sum in the 5th Table, where the apex was λ Herculis.

In the approximation to this point it appeared, that when the line of the parallactic motion of Sirius is made to coincide with its apparent motion, we may soon obtain a certain minimum of the other parallactic motions; but as Sirius is not the star which has the greatest proper motion, it occurred to me that another minimum, obtained from the line in which Arcturus appears to move would be more accurate; for, on account of its great proper motion, we have reason to suppose it more affected than other stars, by the parallax arising from the motion of the sun; and, with a view to this, I soon was led to a point not only in the line of the apparent motion of Arcturus, but equally favourable to Sirius and Procyon, the remaining two stars that have the greatest motions.

If the principle of determining the direction of the solar motion by the stars which have the greatest proper motion be admitted, the following apex must be extremely near the truth; for, an alteration of a few minutes in right ascension or polar distance either way, will immediately increase the required real motion of our stars. Its place is: right ascension 245° 52′ 30″, and north polar distance 40° 22′.

The calculated motions of the same stars by this apex will be as in the following Table, and are delineated in Fig. 4.

Table VII.

Stars.	Angles of the parallactic Motion with the Parallel.	Angles of the apparent with the parallactic Motion.	Least Quantities of the real Motion.
	° ′ ″	° ′ ″	″
Sirius	58 24 56 south-preceding	10 24 44 following	0,20157
Arcturus	55 29 45 south-preceding	0 0 3 preceding	0,00003
Capella	83 44 17 south-preceding	24 40 21 following	0,19358
Lyra	36 28 33 south-following	92 49 30 following	0,32396
Aldebaran	89 48 35 south preceding	13 18 58 following	0,02842
Procyon	59 43 10 south-preceding	9 40 46 preceding	0.20839
		Sum	0″,95595

The sum of the real motions required, with the apex of the solar motion above mentioned, is less in this Table than that in the former by 0″,50343.

In these calculations we have proceeded upon the principle of obtaining the least possible quantity of real motion, by way of coming at the most favourable situation of a solar apex, and have proved that the sum of the observed proper motions of the six principal stars, amounting to 5″,3534, may be the result of a composition of two other motions, and that the real motions of these stars, if they could be reduced to their smallest possible quantities, would not exceed 0″,9559.

But as I do not intend to assert that these real motions can be actually brought down to the low quantities that have been mentioned, it will be necessary to show that the validity of the arguments for establishing the method I have pursued will not be affected by that circumstance. In the first place then, we should consider that although the great proper motions of Arcturus, Procyon, and Sirius, are strong indications of their being affected by parallax, it does not follow, nor is it probable, that the apparent changes of the situation of these stars should be intirely owing to solar motion; on the contrary, we may reasonably expect that their own real motions will have a great share in them. Next to this, it is evident that in the case of parallactic motions the distance of a star from the sun is of material consequence; and as this cannot be assumed at pleasure, we are consequently not at liberty to make the parallactic motion *sp* in Fig. 1, equal to the line *sm* of the same figure; hence it follows, that the real motion of the star cannot be from *m* to *a*, as the foregoing calculations have supposed; but will be from *p* to *a*. It is however very evident, that if *ma* be a minimum, the line *pa*, when *sp* is given, will also be a minimum; and if all the *ma's* in Fig. 4 are minima, it follows also that all the *sp's*, whatever they may be, will give the *pa's* as small as possible: and this is the point that was to be established.

Whatever therefore may be the sum of real motions required to account for the phenomena of proper motions, our foregoing arguments cannot be affected by the result; for, as by observation it is known that proper motions do exist, and since no solar motion can resolve them intirely into parallactic ones, we ought to give the preference to that direction of the motion of the sun which will take away more real motion than any other, and this, as we have shown, will be done when the right ascension of the apex is 245° 52′ 30″, and its north polar distance 40° 22′.

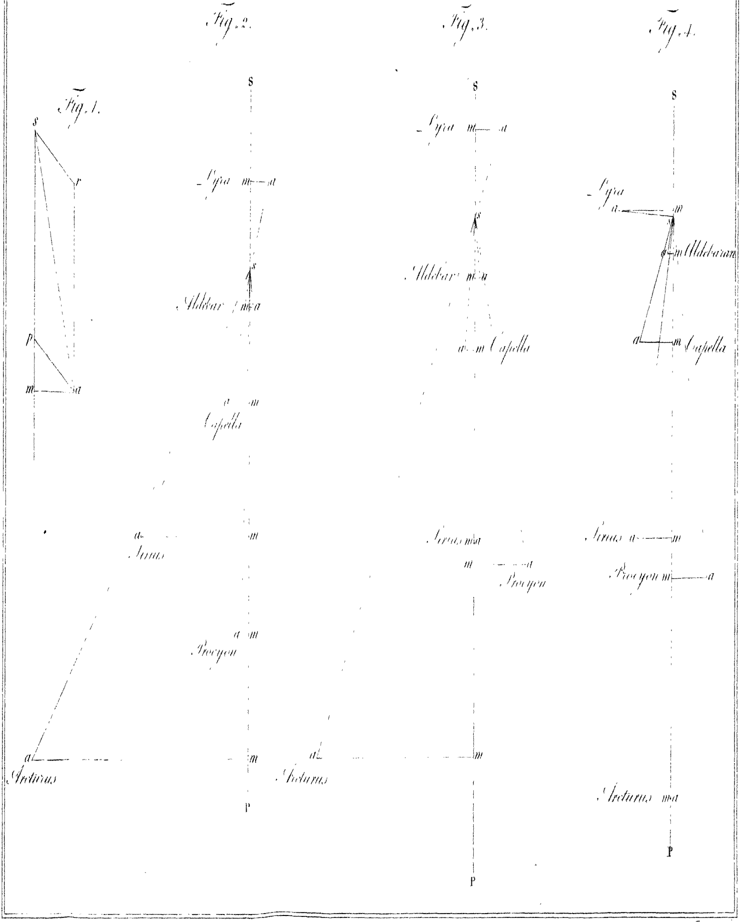

Fig. 1. Fig. 2. Fig. 3. Fig. 4.

Basire sc.

XVII. *On the Reproduction of Buds. By* Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.*

Read May 23, 1805.

MY DEAR SIR,

EVERY tree in the ordinary course of its growth generates, in each season, those buds which expand in the succeeding spring: and the buds thus generated, contain, in many instances, the whole of the leaves which appear in the following summer. But if these buds be destroyed during the winter or early part of the spring, other buds, in many species of trees, are generated, which in every respect perform the office of those which previously existed, except that they never afford fruit or blossoms. This reproduction of buds has not escaped the notice of naturalists; but it does not appear to have been ascertained by them from which, amongst the various substances of the tree, the buds derive their origin.

DU HAMEL conceived that reproduced buds sprang from pre-organized germs; but the existence of such germs has not, in any instance, been proved, and it is well known that the roots, and trunk, and branches, of many species of trees will, under proper management, afford buds from every part of their surfaces; and therefore, if this hypothesis be well founded, many millions of such germs must be annually generated in every large tree; not one of which in the ordinary course of nature will come into action: and as nature, amidst all its exuberance, does not abound in useless productions, the opinions of this illustrious physiologist are, in this case, probably erroneous.

Other naturalists have supposed the buds, when reproduced, to spring from the plexus of vessels which constitutes the internal bark; and this opinion is, I believe, much entertained by modern botanists: it nevertheless appears to be unfounded, as the facts I shall proceed to state will evince.

If the fruit-stalks of the sea cale *(crambe maritima)* be cut off near the ground in the spring, the medullary substance, within that part of the stalk which remains attached to the root, decays; and a cup is thus formed in which water collects in the succeeding winter. The sides of this cup consist of a woody substance, which in its texture and office, and mode of generation, agrees perfectly with the alburnum of trees; and I conceive it to be as perfect alburnum, as the white wood of the oak or elm: and from the interior part of this substance, within the cup, I have frequently observed new buds to be generated in the ensuing spring. It is sufficiently obvious that the buds in this case do not spring from the bark; but it is not equally evident that they might not have sprung from some remains of the medulla.

In the autumn of 1802, I discovered that the potatoe possessed a similar power of reproducing its buds. Some plants of this species had been set, rather late in the preceding spring, in very dry ground, where through want of moisture they vegetated very feebly; and the portions of the old roots remained sound and entire till the succeeding autumn. Being then moistened by rain, many small tubers were generated on the surfaces made by the knife in dividing the roots into cuttings; and the buds of these, in many instances, elongated into runners which gave existence to other tubers, some of which I had the pleasure to send to you.

I have in a former Paper remarked, that the potatoe consists of four distinct substances, the epidermis, the true skin, the bark, and its internal substance, which from its mode of formation, and subsequent office, I have supposed to be alburnous: there is also in the young tubes a transparent line through the center, which is probably its medulla. The buds and runners sprang from the substance which I conceive to be the alburnum of the root, and neither from the central part of it, nor from the surface in contact with the bark. It must, however, be admitted, that the internal substance of the potatoe corresponds more nearly with our ideas of a medullary than of an alburnous substance, and therefore this, with the preceding facts, is adduced to prove only that the reproduced buds of these plants are not generated by the cortical substance of the root: and I shall proceed to relate some experiments on the apple, and pear, and plumb-tree, which I conceive to prove that the reproduced buds of those plants do not spring from the medulla.

Having raised from seeds a very considerable number of plants of each of these species in 1802, I partly disengaged them from the soil in the autumn, by digging round each plant, which was then raised about two inches above its former level. A part of the mould was then removed, and the plants were cut off about an inch below the points where the seed-leaves formerly grew; and a portion of the root, about an inch long, without any bud upon it, remained exposed to the air and light. In the beginning of April, I observed many small elevated points on the bark of these roots, and, removing the whole of the cortical substance, I found that the elevations were occasioned by small protuberances on the surface of the alburnum. As the spring advanced, many minute red points appeared to perforate the bark: these soon assumed the character of buds, and produced shoots, in every respect similar to those which would have sprung from the organized buds of the preceding year. Whether the buds thus reproduced derived any portion of their component parts from the bark or not, I shall not venture to decide; but I am much disposed to believe that, like those of the potatoe, they sprang from the alburnous substance solely.

The space, however, in the annual root, between the medulla and the bark is very small; and therefore it may be contended that the buds in these instances may have originated from the medulla. I therefore thought it necessary to repeat similar experiments on the roots and trunks of old trees, and by these the buds were reproduced precisely in the same manner as the annual roots: and therefore, conceiving myself to have proved in a former Memoir,* that the substance which has been called the medullary process does not originate from the medulla, I must conclude that reproduced buds do not spring from that substance.

I have remarked in a Paper, which you did me the honour to lay before the Royal Society in the commencement of the present year, that the alburnous tubes at their termination upwards invariably join the central vessels, and that these vessels, which appear to derive their origin from the alburnous tubes, convey nutriment, and probably give existence to new buds and leaves. It is also evident, from the facility with which the rising sap is tranferred from one side of a wounded tree to the other, that the alburnous tubes possess lateral, as well as terminal, orifices: and it does not appear improbable that the lateral as well as the terminal orifices of the alburnous tubes may possess the power to generate central vessels; which vessels evidently feed, if they do not give existence to, the reproduced buds and leaves. And therefore, as the preceding experiments appear to prove that the buds neither spring from the medulla nor the bark, I am much inclined to believe that they are generated by central vessels which spring from the lateral orifices of the alburnous tubes. The practicability of propagating some plants from their leaves may seem to stand in opposition to this hypothesis; but the central vessel is always a component part of the leaf, and from it the bud and young plant probably originate.

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* Phil. Trans. of 1803.

I expected to discover in seeds a similar power to regenerate their buds: for the cotyledons of these, though dissimilar in organization, execute the office of the alburnum, and contain a similar reservoir of nutriment, and at once supply the place of the alburnum and the leaf. But no experiments, which I have yet been able to make, have been decisive, owing to the difficulty of ascertaining the number of buds previously existing within the seed. Few, if any, seeds, I have reason to believe, contain less than three buds, one only of which, except in cases of accident, germinates; and some seeds appear to contain a much greater number. The seed of the peach appears to be provided with ten or twelve leaves, each of which probably covers the rudiment of a bud, and the seeds, like the buds of the horse-chesnut, contain all the leaves and apparently all the buds of the succeeding year: and I have never been able to satisfy myself that all the buds were eradicated without having destroyed the base of the plumule, in which the power of reproducing buds probably resides, if such power exists.

Nature appears to have denied to annual and biennial plants (at least to those which have been the subjects of my experiments) the power which it has given to perennial plants to reproduce their buds; but nevertheless some biennials possess, under peculiar circumstances, a very singular resource, when all their buds have been destroyed. A turnip, bred between the English and Swedish variety, from which I had cut off the greater part of its fruit-stalks, and of which all the buds had been destroyed, remained some weeks in an apparently dormant state; after which the first seed in each pod germinated, and bursting the seed-vessel, seemed to execute the office of a bud and leaves to the parent plant, during the short remaining term of its existence, when its preternatural foliage perished with it. Whether this property be possessed by other biennial plants in common with the turnip or not, I am not at present in possession of facts to decide, not having made precisely the same experiment on any other plant.

I will take this opportunity to correct an inference that I have drawn in a former Paper,* which the facts (though quite correctly stated) do not, on subsequent repetition of the experiment, appear to justify. I have stated, that when a perpendicular shoot of the vine was inverted to a depending position, and a portion of its bark between two circular incisions round the stem removed, much more new wood was generated on the lower lip of the wound become uppermost by the inverted position of the branch, than on the opposite lip, which would not have happened had the branch continued to grow erect, and I have inferred that this effect was produced by sap which had descended by gravitation from the leaves above. But the branch was, as I have there stated, employed as a layer, and the matter which would have accumulated on the opposite lip of the wound had been employed in the formation of roots, a circumstance which at that time escaped my attention. The effects of gravitation on the motion of the descending sap, and consequent growth of plants, are, I am well satisfied, from a great variety of experiments, very great; but it will be very difficult to discover any method by which the extent of its operation can be accurately ascertained. For the vessels which convey and impel* the true sap, or fluid from which the new wood appears to be generated, pass immediately from the leaf-stalk towards the root; and though the motion of this fluid may be impeded by gravitation, and it be even again returned into the leaf, no portion of it, unless it had been extravasated, could have descended to the part from which the bark was taken off in the experiment I have described. I am not sensible that in the different Papers which I have had the honour to address to you, I have drawn any other inference which the facts, on repetition of the experiments, do not appear capable of supporting.

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* Phil. Trans of 1803.

I am, &c.

THO^{S}. ANDREW KNIGHT.

Elton,
May 12, 1805.

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* Phil. Trans. of 1804.

XVIII. *Some Account of two Mummies of the Egyptian Ibis, one of which was in a remarkably perfect State.* *By* John Pearson, *Esq. F.R.S.*

Read June 13, 1805.

THE ancient Egyptians were not more remarkable for their attainments in science, than for the extraordinary attention they paid to the bodies of their deceased relatives, preserving their remains by arts which are now either unknown, or imperfectly recorded, and depositing them in subterranean structures, which to this day excite the curiosity and wonder of the philosophic traveller. The practice of embalming was not confined, as is well known, to the conservation of human bodies exclusively; it was likewise employed to protect the remains of several of their sacred animals from that decay and dissolution which usually ensues, on the exposure of animal substances to the action of the earth, or of the atmosphere. We learn from HERODOTUS,[*] that among the different animals which the Egyptians honoured with this peculiar mode of sepulture, were the cat, the ichneumon, the mus araneus terrestris, the ibis, and the hawk; but, whether this be a complete enumeration or not, it is almost impossible, at this period of time, to determine. Mummies of the hawk and of the ibis have been often drawn out of the catacombs; and OLIVIER asserts, that he has not only met with the bones of the mus

[*] Euterpe.

araneus terrestris, but also with those of several of the smaller species of quadrupeds, and that the bones of different animals are not unfrequently contained within the same wrapper.* It is however confidently affirmed by different writers, that the more modern Egyptians have frequently included a single bone of some quadruped within the usual quantity of cloth, which they have artfully taken from some decayed mummy in the catacombs, and then fraudulently sold this sophisticated production as an ancient mummy. Hence, any general conclusions founded on meeting with the bones of other quadrupeds, must be received with diffidence and suspicion.†

The mummies which are taken out of the catacombs of the birds at Saccara, and at Thebes, are included in earthen jars, closed with a cover of the same material. The cloth which envelopes the mummy is sometimes tolerably firm and perfect; but, on removing this, we commonly meet with a quantity of dust, resembling powdered charcoal in its appearance, intermixed with the bones, or the fragments of bones, belonging to the creature which had been contained in it. The decomposition is often so complete, that no traces of the animal remain; but, on other occasions, the intire collection of bones, with the bill of the bird, have been found in a condition sufficiently perfect to construct a skeleton with them. In the fourth volume of the *Annales du Muséum National d'Histoire naturelle*, M. CUVIER has published an interesting memoir on the Ibis, with an engraving of the skeleton of that bird, which had been formed of the bones collected from the catacombs at Thebes. That able naturalist, after comparing the ancient accounts of that celebrated bird with those of the moderns, assigns it a place among the species of curlew, under the name of Numenius Ibis.

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* *Voyage en Egypte*, Tome III. chap viii.

† Phil. Trans. 1794.

The accounts of the mummy of the ibis which have been hitherto made public, were collected from observations made on it in a decayed state: I presume, therefore, that a description of the mummy of an ibis in a condition unusually perfect, may not be unacceptable to the curious. Among the curiosities, natural and artificial, which were collected by the late Major HAYES,* in the years 1802 and 1803, were two small mummies, which he took out of the catacombs at Thebes in Upper Egypt. They were contained in earthen jars, and were enveloped in cloth, similar to those which are brought from Saccara. At the request of his family, I first examined the larger of the two, and found the covering to consist of bandages of cloth, strong and firm, and about three inches broad. The first circumvolutions of the roller separated easily; but, as I proceeded, they adhered more firmly to each other, and were at length so closely cemented together by a resinous-like substance, that I was obliged to divide the folds of the cloth with a strong knife. Each layer of the bandage appeared to have been imbued with some bituminous or resinous substance, in a liquid state, and the roller was farther secured by strong pieces of thread, so that the whole mass was rendered extremely hard and coherent. When I had removed the greater part of the covering, I found that it had contained a bird, which was thickly covered with the same kind of substance that had cemented the different strips of the roller. The examination was now carried on more slowly, by picking out carefully all the loose bituminous matter that could be removed without injuring the mummy; and, after the labour of many hours, I succeeded in displaying the whole bird, as it had been deposited by the embalmer. The operator who had embalmed this bird, had previously disposed its several parts with great order and regularity.

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* This accomplished young gentleman, who served during the late campaign in Egypt, died July 26, 1803, at Rosetta, aged 25 years. By his premature death, his country lost an able officer, and a zealous promoter of the interests of science.

The neck was twisted, so as to place the vertex of the head on the body of the bird, a little to the left side of the sternum. The curved bill, with its concave part turned upwards, descended between the feet, and reached to the extremity of the tail. Each foot, with its four claws turned forwards, was bent upwards, and placed on each side of the head. The wings were brought close to the sides of the body. It was impossible to remove much of the bituminous matter from the back and wings, without injuring the mummy; but I took away a quantity sufficient to show that the plumage was white, the feathers being tipped with dark brown at their extremities; I could not, however, uncover the tail feathers, so as to determine their colour. The bird had attained its full growth; for the quills of one wing, which had suffered some injury in removing the bandage, were in a perfect state: the largest of these quills is delineated, of the natural size, in the annexed Plate. The following are the dimensions of such parts of the Ibis as are accessible.

	Inches.
Length of the bird, from the termination of the neck to the extremity of the tail - - - -	12½
Length of the neck, in which ten vertebræ can be traced - - - - - -	6½
Length of the head and bill, following the curve -	8

	Inches.
Length of the sternum - - - - -	4
From the end of the metatarsal bone to the extremity of the longest toe - - - - - -	7
The longest toe - - - - -	$3\frac{1}{2}$
Width of the body at the shoulders - -	$4\frac{1}{2}$
Circumference of the body, at its thickest part -	$13\frac{1}{2}$

Weight of the mummy, $16\frac{1}{2}$ ounces Troy.

This mummy is in a very firm and intire state, exhibiting no particular marks of decay, although it is probable, that the greater part of 3000 years has elapsed since it was interred: for the destruction of the Egyptian Thebes is of an earlier date than the foundation of any city now existing. The appearance of the mummy renders it probable, that the bird was immersed in the bituminous matter, when it was in a liquid state, and capable of insinuating itself into all the inequalities on the surface of the body; the several folds of the bandage must have been likewise covered with the same varnish: but the animal was certainly not boiled in the liquid, as GREW supposed,* since the feathers are not at all corrugated, nor indeed materially changed from their natural appearance.

The examination of different mummies of the Ibis proves indubitably that the same care has not been used, nor have the same methods been followed, in the preparing of them; but, whether the difference observed depended upon the condition of the bird when it was embalmed, or upon the unequal skill and diligence of the operators, cannot now be ascertained. This, however, is sufficiently evident, that the variety exhibited in their appearance does not depend on the place where the bird was deposited, since many mummies of birds have been taken from the catacombs at Thebes, in as imperfect and decayed a condition as those which have been procured from Saccara.

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* *Musæum Regalis Societatis*, § 1.

I have been favoured with the permission to unroll another mummy of the Ibis, also sent from Thebes by Major HAYES, which had been embalmed in a different manner from that I have already described. The cloth which surrounded it was of a coarser texture, and had not been so thoroughly imbued with bitumen, nor was the roller continued down to the body of the bird; for, when I had removed as much of the bandage as reduced the mummy to about $\frac{2}{3}$ of its original bulk, I found that, instead of circular bands, it was wrapped in several different portions of coarse linen cloth, each of them large enough to contain the whole Ibis. This Ibis was in a decayed state, and had so little coherence, that its several parts separated on handling it: there was a small portion of the neck, with white plumage upon it, remaining, but neither the head, the bill, nor any remains of them, could be discovered. The feathers of this bird are of a dark brown colour, in some parts tipped with white; the neck and the tail have a white plumage, and as much of the tail as could be preserved displayed the tufted appearance delineated in the engraving of M. CUVIER.

Two species of the Ibis, the black and the white, have been noticed by HERODOTUS,* ARISTOTLE,† and PLINY:‡ but PLUTARCH has only mentioned the white Ibis.§ ARISTOTLE and PLINY have contended that the black Ibis was found only at Damietta, (Pelusium,) and that, in all the other parts of Egypt, the white Ibis only was seen. Whether the two birds which I have described present specimens of the black and white Ibis, I cannot presume to determine. The anterior layer of feathers of the Ibis last examined is of a dark colour; but the plumage beneath is white. Many of the dark feathers are not at all marked with white.

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* Euterpe.

† *Hist. Animalium*, lib. ix. c. xxvii.

‡ C. PLINII *Nat. Hist.* lib. x. c. xxx.

§ *De Iside et Osiride.*

The most ancient, and probably the most authentic account which we possess of the Egyptian art of embalming, is delivered by HERODOTUS;[*] and what is offered upon this subject by subsequent writers, seems to have been copied from this early historian. Their narratives relate principally to the conservation of human bodies; and, in the preparing of these, it appears that the contents of the abdomen, at least, were removed by incision, or were corroded by injecting a liquor extracted from the cedar-tree.[†] But it is almost certain, that birds were not previously opened, nor was any art employed to remove the stomach and intestines; for, on examining the interior parts of the dark coloured Ibis, I met with a soft spongy substance, lying quite loose, containing a great number of scarabæi in an imperfect state; these had probably been taken as the food of the bird, and were not digested at the time of its death, but remained in the alimentary canal to the present period. CUVIER also remarks, that he found within the mummy of an Ibis part of the skin and scales of a serpent.

As larvæ of dermestides and other insects have been detected among the dust and bones of a mummy, it may be presumed that the Ibis was not always embalmed in a fresh state; which may indeed account, in part, for the very imperfect condition in which many of these birds are found.

The Ibis was held in great veneration by the Egyptians for

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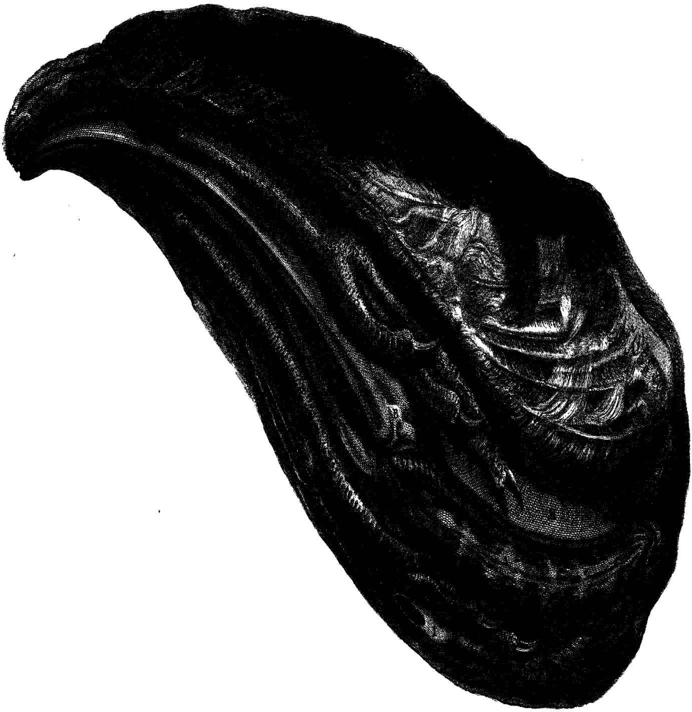
[*] Euterpe.

[†] PANCIROLLUS *Rerum memorab.* pars i. tit. xlii.



Wm Clift delt. Basire sc.

its singular utility in destroying serpents, and other noxious reptiles:[*] hence, the figure of this bird is seen on many monuments of Egyptian antiquity, as an inhabitant of their temples, and an attendant on their sacrifices.[†] It was likewise employed as a symbol in their hieroglyphical writing;[‡] and the punishment of death was inflicted on those who killed this sacred bird. The other extraordinary qualities ascribed to the Ibis by PLINY, PLUTARCH, and some succeeding writers, are either too indistinctly expressed to be quite intelligible, or too obviously absurd to be credible.[§]

EXPLANATION OF PLATE VIII.

A, Vertebræ of the neck.
B, The head.
C, The bill.
D, The tail.
E, The right leg and foot.
F, The left leg and foot.
G, The hind claw bent forwards.
H, The sternum.
I, A quill of the wing feathers.
The whole is represented of the natural size.

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[*] The remark of CICERO on this subject, is perhaps no less true than shrewd: "Ipsi, qui irridentur, Egyptii, nullam belluam, nisi ob aliquam utilitatem, quam ex "ea caperent, consecraverunt." *De Natura Deorum*, lib. i.

[†] *Explication de divers Monumens singuliers*, CALMET.

[‡] *Hieroglyph. Horapollo*, xxxvi. RHODIGIN. *Antiq. Lect.* lib. iv. c. xvi.

[§] C. PLINII *Nat. Hist.* lib. viii. c. xxvii. PLUTARCH. *De Iside*, &c.

XIX. *Observations on the singular Figure of the Planet Saturn.* *By* William Herschel, *LL. D. F. R. S.*

Read June 20, 1805.

THERE is not perhaps another object in the heavens that presents us with such a variety of extraordinary phenomena as the planet Saturn: a magnificent globe, encompassed by a stupendous double ring: attended by seven satellites: ornamented with equatorial belts: compressed at the poles: turning upon its axis: mutually eclipsing its ring and satellites, and eclipsed by them: the most distant of the rings also turning upon its axis, and the same taking place with the farthest of the satellites: all the parts of the system of Saturn occasionally reflecting light to each other: the rings and moons illuminating the nights of the Saturnian: the globe and satellites enlightening the dark parts of the rings: and the planet and rings throwing back the sun's beams upon the moons, when they are deprived of them at the time of their conjunctions.

It must be confessed that a detail of circumstances like these, appears to leave hardly any room for addition, and yet the following observations will prove that there is a singularity left, which distinguishes the figure of Saturn from that of all the other planets.

It has already been mentioned on a former occasion, that so far back as the year 1776 I perceived that the body of Saturn was not exactly round; and when I found in the year 1781 that it was flattened at the poles at least as much as Jupiter, I was insensibly diverted from a more critical attention to the rest of the figure. Prepossessed with its being spheroidical, I measured the equatorial and polar diameters in the year 1789, and supposed there could be no other particularity to remark in the figure of the planet. When I perceived a certain irregularity in other parts of the body, it was generally ascribed to the interference of the ring, which prevents a complete view of its whole contour; and in this error I might still have remained, had not a late examination of the powers of my 10-feet telescope convinced me that I ought to rely with the greatest confidence upon the truth of its representations of the most minute objects I inspected.

The following observations, in which the singular figure of Saturn is fully investigated, contain many remarks on the rest of the appearances that may be seen when this beautiful planet is examined with attention; and though they are not immediately necessary to my present subject, I thought it right to retain them, as they show the degree of distinctness and precision of the action of the telescope, and the clearness of the atmosphere at the time of observation.

April 12, 1805. With a new 7-feet mirror of extraordinary distinctness, I examined the planet Saturn. The ring reflects more light than the body, and with a power of 570 the colour of the body becomes yellowish, while that of the ring remains more white. This gives us an opportunity to distinguish the ring from the body, in that part where it crosses the disk, by means of the difference in the colour of the reflected light. I saw the quintuple belt, and the flattening of the body at the polar regions; I could also perceive the vacant space between the two rings.

The flattening of the polar regions is not in that gradual manner as with Jupiter, it seems not to begin till at a high latitude, and there to be more sudden than it is towards the poles of Jupiter. I have often made the same observation before, but do not remember to have recorded it any where.

April 18; 10-feet reflector, power 300. The air is very favourable, and I see the planet extremely well defined. The shadow of the ring is very black in its extent over the disk south of the ring, where I see it all the way with great distinctness.

The usual belts are on the body of Saturn; they cover a much larger zone than the belts on Jupiter generally take up, as may be seen in the figure I have given in Plate IX.; and also in a former representation of the same belts in 1794.*

The figure of the body of Saturn, as I see it at present, is certainly different from the spheroidical figure of Jupiter. The curvature is greatest in a high latitude.

I took a measure of the situation of the four points of the greatest curvature, with my angular micrometer, and power 527. When the cross of the micrometer passed through all the four points, the angle which gives the double latitude of two of the points, one being north the other south of the ring, or equator, was 93° 16′. The latitude therefore of the four points is 46° 38′; it is there the greatest curvature takes place. As neither of the cross wires can be in the parallel, it makes the measure so difficult to take, that very great accuracy cannot be expected.

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* See Phil. Trans. for 1794, Table VI. page 32.

The most northern belt comes up to the place where the ring of Saturn passes behind the body, but the belt is bent in a contrary direction being concave to the north, on account of its crossing the body on the side turned towards us, and the north pole being in view.

There is a very dark, but narrow shadow of the body upon the following part of the ring, which as it were cuts off the ring from the body.

The shadow of the ring on the body, which I see south of the ring, grows a little broader on both sides near the margin of the disk.

The division between the two rings is dark, like the vacant space between the ansæ, but not black like the shadow I have described.

There are four satellites on the preceding side near the ring; the largest and another are north-preceding; the other two are nearly preceding.

April 19. I viewed the planet Saturn with a new 7-feet telescope, both mirrors of which are very perfect. I saw all the phenomena as described last night, except the satellites, which had changed their situation; four of them being on the following side. This telescope however is not equal to the 10-feet one.

The remarkable figure of Saturn admits of no doubt: when our particular attention is once drawn to an object, we see things at first sight that would otherwise have escaped our notice.

10-feet reflector, power 400. The night is beautifully clear, and the planet near the meridian. The figure of Saturn is somewhat like a square or rather parallelogram, with the four corners rounded off deeply, but not so much as to bring it to a spheroid. I see it in perfection.

The four satellites that were last night on the preceding, are now on the following side, and are very bright.

I took a measure of the position of the four points of the greatest curvature, and found it 91° 29′. This gives their latitude 45° 44′,5. I believe this measure to be pretty accurate. I set first the fixed thread to one of the lines, by keeping the north-preceding and south-following two points in the thread; then adjusted the other thread in the same manner to the south-preceding and north-following points.

May 5, 1805. I directed my 20-feet telescope to Saturn, and, with a power of about 300, saw the planet perfectly well defined, the evening being remarkably clear. The shadow of the ring on the body is quite black. All the other phenomena are very distinct.

The figure of the planet is certainly not spheroidical, like that of Mars and Jupiter. The curvature is less on the equator and on the poles than at the latitude of about 45 degrees. The equatorial diameter is however considerably greater than the polar.

In order to have the testimony of all my instruments, on the subject of the structure of the planet Saturn, I had prepared the 40-feet reflector for observing it in the meridian. I used a magnifying power of 360, and saw its form exactly as I had seen it in the 10 and 20-feet instruments. The planet is flattened at the poles, but the spheroid that would arise from this flattening is modified by some other cause, which I suppose to be the attraction of the ring. It resembles a parallelogram, one side whereof is the equatorial, the other the polar diameter, with the four corners rounded off so as to leave both the equatorial and polar regions flatter than they would be in a regular spheroidical figure.

The planet Jupiter being by this time got up to a considerable altitude, I viewed it alternately with Saturn in the 10-feet reflector, with a power of 500. The outlines of the figure of Saturn are as described in the observation of the 40-feet telescope: but those of Jupiter are such as to give a greater curvature both to the polar and equatorial regions than takes place at the poles or equator of Saturn which are comparatively much flatter.

May 12. I viewed Saturn and Jupiter alternately with my large 10-feet telescope of 24 inches aperture; and saw plainly that the former planet differs much in figure from the latter.

The temperature of the air is so changeable that no large mirror can act well.

May 13. 10-feet reflector, power 300. The shadows of the ring upon the body, and of the body upon the ring, are very black, and not of the dusky colour of the heavens about the planet, or of the space between the ring and planet, and between the two rings. The north-following part of the ring, close to the planet, is as it were cut off by the shadow of the body: and the shadow of the ring lies south of it, but close to the projection of the ring.

The planet is of the form described in the observation of the 40-feet telescope; I see it so distinctly that there can be no doubt of it. By the appearance, I should think the points of the greatest curvature not to be so far north as 45 degrees.

The evening being very calm and clear, I took a measure of their situation, which gives the latitude of the greatest curvature 45° 21′. A second measure gives 45° 41′.

Jupiter being now at a considerable altitude, I have viewed it alternately with Saturn. The figure of the two planets is decidedly different. The flattening at the poles and on the equator of Saturn is much greater than it is on Jupiter, but the curvature at the latitude of from 40 to 48° on Jupiter is less than on Saturn.

I repeated these alternate observations many times, and the oftener I compared the two planets together, the more striking was their different structure.

May 26. 10-feet reflector. With a parallel thread micrometer and a magnifying power of 400, I took two measures of the diameter of the points of greatest curvature. A mean of them gave 64,3 divisions = 11″,98. After this, I took also two measures of the equatorial diameter, and a mean of them gave 60,5 divisions = 11″,27; but the equatorial measures are probably too small.

To judge by a view of the planet, I should suppose the latitude of the greatest curvature to be less than 45 degrees. The eye will also distinguish the difference in the three diameters of Saturn. That which passes through the points of the greatest curvature is the largest; the equatorial the next, and the polar diameter is the smallest.

May 27. The evening being very favourable, I took again two measures of the diameter between the points of greatest curvature, a mean of which was 63,8 divisions = 11″,88. Two measures of the equatorial diameter gave 61,3 divisions = 11″,44.

June 1. It occurred to me that a more accurate measure might be had of the latitude in which the greatest curvature takes place, by setting the fixed thread of the micrometer to the direction of the ring of Saturn, which may be done with great accuracy. The two following measures were taken in this manner, and are more satisfactory than I had taken before. The first gave the latitude of the south-preceding point of greatest curvature 43° 26′; and the second 43° 13′. A mean of the two will be 43° 20′.

June 2. I viewed Jupiter and Saturn alternately with a magnifying power of only 300, that the convexity of the eye-glass might occasion no deception, and found the form of the two planets to differ in the manner that has been described.

With 200 I saw the difference very plainly; and even with 160 it was sufficiently visible to admit of no doubt. These low powers show the figure of the planets perfectly well, for as the field of view is enlarged, and the motion of the objects in passing it lessened, we are more at liberty to fix our attention upon them.

I compared the telescopic appearance of Saturn with a figure drawn by the measures I have taken, combined with the proportion between the equatorial and polar diameters determined in the year 1789;* and found that, in order to be a perfect resemblance, my figure required some small reduction of the longest diameter, so as to bring it nearly to agree with the measures taken the 27th of May. When I had made the necessary alteration, my artificial Saturn was again compared with the telescopic representation of the planet, and I was then satisfied that it had all the correctness of which a judgment of the eye is capable. An exact copy of it is given in Plate IX. The dimensions of it in proportional parts are,

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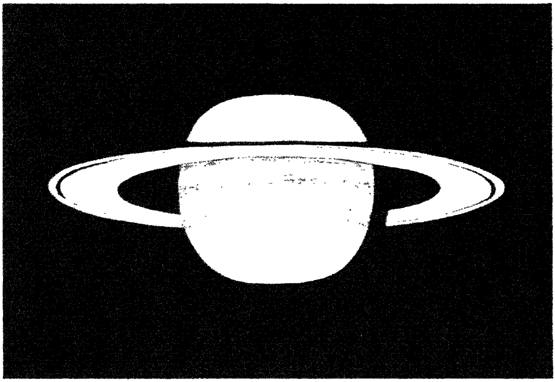
* See Phil. Trans. for 1790, page 17.

The diameter of the greatest curvature	36
The equatorial diameter	35
The polar diameter	32
Latitude of the longest diameter	43° 20′.

The foregoing observations of the figure of the body of Saturn will lead to some intricate researches, by which the quantity of matter in the ring, and its solidity, may be in some measure ascertained. They also afford a new instance of the effect of gravitation on the figure of planets; for in the case of Saturn, we shall have to consider the opposite influence of two centripetal and two centrifugal forces: the rotation of both the ring and planet having been ascertained in some of my former Papers.



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XX. *On the magnetic Attraction of Oxides of Iron. By* Timothy Lane, *Esq. F. R. S.*

Read June 20, 1805.

Having found by experiment, that hardened iron is not so readily attracted by the magnet as soft iron, and that needles are inferior to iron wire as indexes to Six's thermometer, I was proceeding to other comparative experiments, when I received the Second Part of last year's Philosophical Transactions, in which I saw an Analysis of magnetical Pyrites, with Remarks on Sulphurets of Iron, by Mr. Hatchett.

This Paper led me to examine what magnetical properties iron possessed, when free from inflammable matter. For this purpose I obtained a precipitate of iron, prepared and sold at Apothecaries' Hall by the name of *Ferrum præcipitatum*. Mr. Moore, the chemical operator, informed me, that he prepared it by dissolving twelve pounds of sulphate of iron in twenty-four gallons of distilled water, and then adding eight ounces of sulphuric acid to render the solution more complete. Twelve pounds of purified kali were mixed with the solution: the precipitate was well washed with hot distilled water, and then carefully dried. This precipitate is similar to the sediment of chalybeate waters, and affords no magnetic particles; nor, when exposed to a continued clear red heat, does it suffer any alteration beyond the acquirement of a darker colour. But if any smoke or flame has access to it, then magnetic particles mable substances mixed with them do not render them magnetic, until they are by heat chemically combined with the oxides, and that when the combustible substance is again separated by heat, the oxides return to their unmagnetic state. That magnetic oxides cannot be distinguished from calcined oxides by their colour. I entertain a hope, however, that this subject may be found worthy of the accurate investigation of some other member of this learned Society.

XXI. *Additional Experiments and Remarks on an artificial Substance, which possesses the principal characteristic Properties of* Tannin. *By* Charles Hatchett, *Esq. F. R. S.*

Read June 27, 1805.

§ I.

When I had ascertained that carbonaceous substances, whether vegetable, animal, or mineral, were capable of being converted into a product, which, by its effects on earthy and metallic solutions, on dissolved gelatine, and on skin, resembled the natural vegetable principle called *Tannin,* I was at first inclined to give it the name of artificial or factitious *tannin;* but some eminent chemists of this country, for whose opinions I have the highest respect, considered this name as objectionable; for although the artificial substance resembles *tannin* in the particulars above stated, yet in one character there appears to be a very considerable difference, namely, the effect of nitric acid; for by this, the artificial substance is *produced,* whilst the varieties of natural *tannin* are *destroyed.* Such an objection, sanctioned by such authority, induced me to alter the title of my Paper, and to expunge the word *tannin* wherever it had been applied to the artificial product.

In order to satisfy myself more fully on this point, I have, since the communication of my former Paper, made a few experiments on the comparative effects produced by nitric acid on those substances which contain the most notable quantities of *tannin*, and of these I shall now give a succinct account, and shall also cursorily notice other experiments, in which a tanning substance has been produced, under circumstances different, in some measure, from those which have been already described.

§ II.

Although I cannot as yet assert, that the artificial tanning product is absolutely indestructible when repeatedly distilled with different portions of nitric acid, yet the following experiments will prove, that the destructibility of it by this method must at least be a work of considerable time and difficulty.

1. Twenty grains of this substance were dissolved in half an ounce of strong nitric acid, the specific gravity of which was 1.40. The solution was then subjected to distillation until the whole of the acid had come over, after which, it was poured back upon the residuum, and the distillation was thus repeated three times.

Care was taken not to overheat the residuum, and this, when examined, did not appear to have suffered alteration in any of its properties.

2. Ten grains of the artificial tanning substance, mixed with ten grains of white sugar, were dissolved in half an ounce of nitric acid, and the whole was distilled to dryness.

The residuum being then dissolved in boiling distilled water, and examined by solution of gelatine and other reagents, was found to be unchanged in every respect.

3. This resembled the former, only that gum arabic was employed in the place of sugar. The result was the same.

4. A quantity of dissolved isinglass was precipitated by a

solution of the artificial tanning substance, and the precipitate having been well washed with hot distilled water, was afterwards gradually dried. It was then digested in strong nitric acid, which after some time acted powerfully upon it; much nitrous gas was evolved, and a dark brown solution was formed. This was evaporated to dryness, and after having been completely dissolved in boiling distilled water, was examined by nitrate of lime, acetite of lead, muriate of tin, and solution of isinglass, all of which formed copious precipitates, similar in every respect to those produced by the artificial tanning substance, which had not been subjected to the above described process.

5. A portion of the precipitate, formed by isinglass and the tanning substance, was dissolved in pure muriatic acid, and was afterwards evaporated to dryness. Boiling distilled water dissolved only a small part, and the solution, which was of a dark beer colour, did not precipitate gelatine, although it acted upon muriate of tin, and sulphate of iron; for with the former it produced an ash-coloured precipitate, and with the latter a slight deposit of a reddish-brown colour.

6. As so small a part of the precipitated isinglass had been thus rendered soluble in boiling water, the residuum was treated with nitric acid, as in experiment 4, after which, being evaporated to dryness, it was found to be completely soluble in water, and precipitated gelatine as copiously as at first.

7. I dissolved 20 grains of the pure tanning substance in about half an ounce of muriatic acid; but, after distillation to dryness, the residuum in every respect appeared to be unchanged.

In addition to the above experiments may be added, that the solutions of the artificial tanning substance seem to be completely imputrescible, neither do they ever become mouldy like the infusions of galls, sumach, catechu, &c.

Having thus ascertained the very unchangeable nature of this substance, I made the following comparative experiments on galls, sumach, Pegu cutch, kascutti, common cutch, and oak bark.

8. Twenty grains of powdered galls were dissolved in half an ounce of the strong nitric acid; the solution was then evaporated to dryness, and the residuum dissolved in boiling water. This did not produce the smallest effect on dissolved gelatine.

9. A strong infusion of galls evaporated to dryness, and treated as above, was totally deprived of the tanning property.

10. Isinglass precipitated by the infusion of galls, was dissolved in nitric acid, and examined as in experiment 4, but no trace of *tannin* could be discovered.

11. Twenty grains of sumach were dissolved in half an ounce of the strong nitric acid, and treated as in experiment 8, after which it appeared that the *tannin* was destroyed.

12. Twenty graints of Pegu cutch (which contains a considerable quantity of mucilage) were subjected to a similar process, by which much oxalic acid was obtained, but every vestige of *tannin* was obliterated.

13. Twenty grains of the catechu called Kascutti afforded results similar to the above.

14. Twenty grains of the common cutch or catechu being dissolved in nitric acid, evaporated to dryness, dissolved in water, and examined by solution of isinglass, rendered the latter

turbid, a tenacious film was deposited, which was insoluble in boiling water, and was evidently composed of gelatine and *tannin*.

15. Twenty grains of prepared oak bark, by the like treatment, afforded a solution in water, which still acted in some measure upon gelatine, as it caused a solution of isinglass to become slightly turbid, and a film completely insoluble in boiling water was, as in experiment 14, deposited on the sides and bottom of the vessel.

16. Infusions were prepared as nearly as possible of equal strength from galls, sumach, shavings of oakwood, oak bark, and the artificial tanning substance; half an ounce in measure of each was then put into separate glasses, and one drachm in measure of the strong nitric acid was added.

The different infusions were then examined by solution of isinglass, and I found that those of galls, sumach, and oak wood, were not rendered turbid, whilst the contrary happened to the infusions of oak bark, and of the artificial substance; for these continued to precipitate gelatine, until four drachms or half an ounce of the nitric acid had been added to each half ounce of the infusion.

When the results of these experiments are compared, they seem to establish. that although the artificial product is by much the most indestructible of all the tanning substances, yet there is some difference in this respect even between the varieties of natural *tannin*; and that common catechu, and the *tannin* of oak bark, resist the effects of nitric acid much longer than galls, sumach, kascutti, and Pegu cutch. The last, as I have observed, is replete with mucilage, and by nitric acid yields a large quantity of oxalic acid; it also appears to be the most destructible of all the varieties of catechu, and on this account I attempted, although without success, to promote the destruction of the properties of the artificial substance, by adding gum arabic in one case, and sugar in another, to different portions, previous to exposing it to the action of nitric acid. I am however, convinced, that the presence of gum or mucilage in natural substances which contain *tannin*, renders this more speedily destructible by nitric acid, and I shall soon have occasion to notice some experiments which tend to prove, that the presence of gum or mucilage in certain bodies, also prevents or impedes more or less the formation of the artificial tanning substance. The cause of this difference I am inclined to suspect is, that in those bodies the gum or mucilage is not simply mixed, but is present in a state of chemical combination, by which, certain modifications produced by the action of nitric acid upon the elementary principles of the original substance become facilitated.

§ III.

A. When sulphuric acid was added to a solution of the artificial tanning substance, the latter became turbid, and a copious brown precipitate subsided, which was soluble in boiling distilled water, and then was capable of precipitating gelatine.

B. The same effect was produced by muriatic acid; so that in these particulars, the artificial tanning substance was found to resemble precisely the *tannin* of galls and of other natural substances.*

C. Carbonate of potash, when added to a solution of the artificial tanning substance, deepens the colour, after which, the solution becomes turbid and deposits a brown magma.

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* Mr. DAVY on the Constituent Parts of Astringent Vegetables. Phil. Trans. 1803, p. 240, 241.

D. Five grains of the dry substance were dissolved in half an ounce of strong ammonia; the whole was then evaporated to dryness, and being dissolved in water, was found not to precipitate gelatine, unless a small portion of muriatic acid was previously added.

E. Another portion of the same substance which had been dissolved in ammonia was evaporated in a long necked matrass, and was kept in very hot sand during half an hour; at first some ammonia arose, and afterwards a yellow liquor which had the odour of burned horn. The residuum was then examined, and was found to be nearly insoluble in water, to which it only communicated a slight yellow tinge.

F. It is remarkable, that the dry artificial tanning substance, although prepared from vegetable matter, should, when placed on a hot iron, emit an odour very analogous to burned animal substances, such as horn, feathers, &c.; this I found also to be the case in the experiment which has been related, and I was desirous therefore to ascertain more accurately the effects of heat on this substance when distilled in close vessels.

I took some very pure vegetable charcoal which had been exposed to a red heat in a retort for more than an hour, and by nitric acid converted it into the artificial tanning substance.

Twenty grains of this, rendered as dry as possible, were put into a small glass retort, to which a proper apparatus terminating in a jar filled with quicksilver and inverted in a mercurial trough was adapted. The retort was placed in a small furnace, and was gradually heated by a charcoal fire until the bulb became red hot.

When the retort became warm, and after the expulsion of the atmospheric air, a very small portion of water arose, which settled like dew on the sides of the vessels; this was succeeded by a little nitric acid, from which the tanning substance had not been completely freed, and soon after a yellowish liquor came over, which was in so very small a quantity as only to stain the upper part of the neck of the retort: as nothing more seemed to be produced, I then raised the fire, when suddenly the vessels were filled with a white cloud, and so great a portion of gas was almost explosively produced, as to overset the jar; this gas, by its odour, appeared to be ammonia, which in the first instance had formed the white cloud, by combining with the vapour of the nitric acid with which the vessels were previously filled.* Another jar was speedily placed in the room of that which had been overturned, and a quantity of gas was slowly collected; this proved to be carbonic acid, excepting a very small part, which was not taken up by solution of caustic potash, and which as far as the smallness of the quantity would permit to be determined, appeared to be nitrogen gas. There remained in the retort a very bulky coal, which weighed eight grains and a half; this by incineration yielded one grain and a half of brownish white ashes, which consisted principally of lime, but whether any alkali was also present I cannot positively assert, as the trace which I thought I discovered of it was very slight.

I shall for the present postpone any remarks upon this experiment, as I wish to proceed in the account of others which have been made on the artificial tanning substance.

G. Fifty grains of this substance were dissolved in four ounces of water, and were afterwards precipitated by dissolved isinglass, eighty-one grains of which became thus combined with forty-six grains of the tanning substance.

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* After the experiment the receiver was found to be thinly coated with a white saline crust.

The remaining portion of the latter was not precipitated, and was therefore separated by a filter, and evaporated to dryness. It then appeared in the state of a light brittle substance of a pale cinnamon brown colour; and it is very singular, that although charcoal is an inodorous body, and although the artificial tanning substance, when properly prepared, is likewise devoid of smell, (unless a certain pungent sensation which may be perceived upon first opening a bottle containing the powder after agitation should be so termed, but which seems rather to be a mechanical effect) yet this substance possessed a strong odour not very unlike prepared oak bark, and this odour became much more perceptible when the substance was put into water, in which it immediately dissolved. The solution was extremely bitter, and acted but slightly on dissolved isinglass, with which, however, it formed some flocculi; with sulphate of iron it produced a brown precipitate: with muriate of tin one which was blackish brown; nitrate of lime had not any effect; but acetite of lead occasioned a very copious precipitate of a pale brown colour. This substance therefore appeared to be a portion of the tanning matter so modified, as partly to possess the characters of extract.*

Other experiments were made on the tanning substance prepared from various bodies, which by the dry and by the humid way had been previously reduced to the state of coal; but these I shall here omit, and shall pass to the description of a series of experiments, by which I obtained a variety of the artificial tanning substance in a way different from that which has been related, and with which I was unacquainted when my former paper was written.

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* When added to a solution of carbonate of ammonia, it produced some effervescence, but its peculiar vegetable odour did not suffer any diminution.

§ IV.

I made several unsuccessful attempts to form the artificial substance by means of oxymuriatic acid, and it therefore appeared certain, that although a variety of the tanning matter could be produced by the action of sulphuric acid on resinous substances, yet the most effective agent was nitric acid, which readily formed it when applied to any sort of coal.

But I nevertheless suspected, that possibly this substance, or something similar to it, might be produced without absolutely converting vegetable bodies into coal; for it seemed, as I have observed in my former paper, that this only served to separate the carbon in a great measure from the other elementary principles (excepting oxygen) which were combined with it in the original substance, and thus to expose it more completely to the effects of the nitric acid, as well as to prevent the formation of the various acid products, which are so constantly afforded by the organized substances when thus treated. At first I had some thoughts of employing touchwood in this experiment, but not being able immediately to procure any, it occurred to me, that indigo might probably answer the purpose: for from some experiments made by myself, as well as from those described by BERGMAN,* I well knew that the proportion of carbon in this substance is very considerable. The following experiment was therefore made.

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* *Analysis Chemica Pigmenti Indici.* Opuscula BERG. Tom. V. p. 36.

1. On one hundred grains of fine indigo which had been put into a long matrass, one ounce of nitric acid diluted with an equal quantity of water was poured, and, as the action of the acid was almost immediate and extremely violent, another ounce of water was added. When the effervescence had nearly subsided, the vessel was placed in a sand-bath during several days, until the whole of the liquid was evaporated.

On the residuum, which was of a deep orange colour, three ounces of boiling distilled water were poured, by which a considerable part was dissolved.

The colour of the solution was a most beautiful deep yellow, and the bitter flavour of it surpassed in intensity that of any substance in my recollection; it was examined by the following reagents.

Sulphate of iron produced a slight pale yellow precipitate.

Nitrate of lime only rendered it a little turbid, after which, a small portion of white powder subsided, which had the characters of oxalate of lime.

Muriate of tin produced a copious white precipitate, which afterwards changed to a yellowish-brown.

Acetite of lead formed a very beautiful deep lemon-coloured precipitate, which possibly may prove useful as a pigment.

Ammonia rendered the colour much deeper, after which the liquor became turbid, and a large quantity of fine yellow spiculated crystals was deposited, which being dissolved in water, did not precipitate lime from its solutions.

The flavour of these crystals was very bitter, and I suspect them to be composed of ammonia combined with the bitter principle first noticed by WELTHER.*

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* THOMSON's System of Chemistry, 2d edit. Vol. IV. p. 246.

Lastly, when dissolved isinglass was added to the yellow solution of indigo, it immediately became very turbid, and a bright yellow substance was gradually deposited, and coated the sides of the glass jar with a tough elastic film, which was insoluble in boiling water, and possessed the characters of gelatine combined with tanning matter.

By this experiment I therefore ascertained, that a variety of the artificial tanning substance could be formed without previously converting the vegetable body into coal; and I have since discovered, that although indigo more readily yields this substance than most of the other vegetable bodies, yet in fact, very few of these can be regarded as exceptions, when subjected to repeated digestion and distillation with nitric acid.

2.—A. In my former Paper I have stated, that common resin, when treated with nitric acid, yielded a pale yellow solution with water, which did not precipitate gelatine, and that it was requisite to develope part of the carbon in the state of coal by sulphuric acid, before any of the tanning substance could be produced; but having again made some of these experiments, I repeated the abstraction of nitric acid several times, and then observed, that the solution of resin in water acted upon gelatine similar to the solution of indigo which has been described, and formed a tough yellow precipitate, which was insoluble in boiling water.

With other reagents the effects were as follows.

Sulphate of iron, after 12 hours, formed a slight yellow precipitate.

Nitrate of lime did not produce any effect.

Muriate of tin, after 12 hours, afforded a pale brown precipitate

And acetite of lead immediately formed a very abundant precipitate of a yellowish white colour.

I repeated this experiment on common resin, and remarked, that during each distillation nitrous gas was produced, whilst the strength of the acid which came over was diminished; the cause therefore of the change in the properties of the resin seemed to me very evident, and I was induced to extend the experiments to various resinous and other substances; but as the process was uniformly the same, I shall only notice the principal effects.

B. Stick lac, when separated from the twigs, and treated as above described, copiously precipitated gelatine.

C. Balsam of Peru during the process afforded some benzoic acid, and gelatine was precipitated by the aqueous solution.

D. Benzoin also, after the sublimation of some benzoic acid, yielded a residuum, which with water formed a pale yellow solution, of a very bitter flavour.

This solution with sulphate of iron afforded a slight pale yellow precipitate.

With nitrate of lime not any effect was produced.

The solution with muriate of tin became turbid, and a small quantity of brownish-white precipitate subsided.

Acetite of lead immediately produced a copious pale yellow precipitate.

And solution of isinglass formed a dense yellow precipitate, which was insoluble in boiling water.

E. Balsam of Tolu, like Balsam of Peru, and Benzoin, afforded some benzoic acid; and the residuum being dissolved in water, was found to precipitate gelatine.

F. As the results of the experiments on dragon's blood were somewhat remarkable, I shall here more particularly relate them. One hundred grains of pure dragon's blood, reduced to powder, were digested in a long matrass with one ounce of strong nitric acid; the colour immediately changed to deep yellow, much nitrous gas was evolved, and to abate the effervescence, one ounce of water was added. The digestion was continued until a deep yellow dry mass remained, and the matrass being still kept in the sand-bath, a brilliant feather-like sublimate arose, which weighed rather more than six grains, and had the aspect, odour, and properties of benzoic acid.*

The residuum was of a brown colour, and with water formed a golden yellow-coloured solution, which by nitrate of lime was not affected.

With sulphate of iron it afforded a brownish-yellow precipitate.

With muriate of tin the result was similar.

With acetite of lead a lemon-coloured precipitate was produced.

Gold was precipitated by it in the metallic state, whilst the glass vessel acquired a tinge of purple:

And dissolved isinglass produced a deep yellow deposit, which was insoluble in boiling water.

A portion of the same dragon's blood was simply exposed to heat in the same matrass, but not any appearance of benzoic acid could be discovered. I am therefore induced to believe, that in the first experiment it was obtained as a product, and not as an educt, a fact which as yet has not been suspected.

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* According to these experiments, dragon's blood ought to be arranged with benzoin and the balsams, but as the samples of this drug are not always precisely similar, it would be proper to examine every variety. That which was employed in my experiments, was a porous mass of a dark red, and was sent to me by Messrs. ALLEN and HOWARD, of Plough Court, in Lombard Street.

G. Gum ammoniac afforded a brownish yellow solution, the flavour of which was very bitter and astringent.

By sulphate of iron, this solution only became of a darker colour, but did not form any precipitate.

Nitrate of lime rendered it turbid, and produced a slight precipitate.

Muriate of tin formed a copious yellow precipitate.

Acetite of lead produced a similar effect:

And gelatine yielded a bright yellow deposit, which was completely insoluble by boiling water.

H. Asa fœtida yielded a solution which also precipitated gelatine like the substances above described.

I. Solutions of elemi, tacamahac, olibanum, sandarach, copaiba, mastich, myrrh, gamboge, and caoutchouc, were next examined, but these, although they precipitated the metallic solutions, did not affect gelatine. It is possible, however, that they might have produced this effect, had they been subjected to a greater number of repetitions of the process.

K. Sarcocol, in its natural state, as well as the gum separated from it by water, when treated with nitric acid, did not precipitate gelatine; but produced effects on the metallic solutions similar to the above mentioned substances.

L. Gum arabic afforded oxalic acid, but not any of the tanning matter.

M. Tragacanth yielded an abundance of saclactic acid, of oxalic, and of malic acid, but not the smallest vestige of the artificial tanning substance.

N. Manna, when treated with nitric acid in the way above described, afforded oxalic acid, part of which was sublimed in the neck of the vessel.

The residuum with water formed a brown solution, which yielded a pale yellow precipitate with sulphate of iron.

Muriate of tin produced a pale brown precipitate.

Acetite of lead formed one of a brownish-white hue.

Lime was copiously precipitated from the nitrate of lime in the state of oxalate; but not the smallest effect was produced on solution of isinglass.

O. Liquorice however afforded a different result; for, although the solution after the process with nitric acid resembled in appearance that which was yielded by manna, yet the effects were not the same.

Sulphate of iron, after twelve hours, produced a slight brown precipitate.

Muriate of tin had a similar effect.

Acetite of lead formed a brownish-red deposit.

Nitrate of lime also occasioned a brown precipitate:

And solution of isinglass rendered it very turbid, and produced a yellowish-brown precipitate, which was insoluble in boiling water, and possessed all the other characters of gelatine combined with the tanning substance.

P. Guaiacum, the properties of which are so singular in many respects, afforded results (when treated with nitric acid in the manner which has been described) different from the resins, although its external and general characters seem to indicate that it appertains to those bodies.

Nitric acid acted upon it with great vehemence, and speedily dissolved it. The residuum which was afterwards obtained, was also found to be almost totally soluble in water, and the solution acted on the metallic salts like those which have already been noticed, but with gelatine it formed only a very slight precipitate, which was immediately dissolved by boiling water; and the remainder of the solution being evaporated, yielded a very large quantity of crystallized oxalic acid; so that in this respect guaiacum was found to resemble the gums, and to be totally unlike the resins.*

§ V.

As many vegetable substances when roasted, yield by decoction a liquid, which in appearance much resembles the artificial tanning matter when dissolved in water; I roasted some of the common dried peas, horse-beans, barley, and wheat flour, the decoctions of which however did not afford any precipitate by solution of isinglass.

Even the decoction of coffee did not yield any precipitate by this method, until several hours had elapsed, and I found that the precipitate so formed was permanently soluble in boiling water. But to explain this, we must recollect, it is extremely probable, that some peculiar nicety is required in the roasting of such bodies before the tanning substance can be developed; and this seems to be corroborated by some experiments which I made on the decoction of a sort of coffee prepared from the chicoreé (I suppose endive) root, which was given me by

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* The properties of guaiacum which have been described, as well as those which were previously known, appear to indicate, that it is a peculiar substance of a nature distinct from the resins, balsams, and even the gum resins.

So remarkable indeed is this substance, that an accurate series of experiments on the whole of its properties may justly be placed amongst the chemical desiderata.

Sir JOSEPH BANKS; for although this decoction did not afford an immediate precipitate with solution of gelatine, and although the precipitate was also apparently dissolved by boiling water, yet upon cooling, the same precipitate was reproduced in its original state. I am therefore inclined to believe, that the tanning substance is really developed in many of the vegetable bodies by heat, but that a certain degree of temperature, not very easy to determine, is absolutely requisite for this purpose.

Before I conclude this section, it may be proper to observe, that when a small quantity of nitric acid was added to any of the above-mentioned decoctions, and when these had been subsequently evaporated to dryness, and afterwards dissolved in distilled water, they were converted into a tanning substance perfectly similar to that which is produced by the action of nitric acid on the varieties of coal.

§ VI.

In the preceding Paper, a variety of the tanning substance was slightly noticed, which was formed by the action of sulphuric acid upon common resin, elemi, amber, &c. &c. and as an instance has occurred of the formation of the same substance from camphor, accompanied by circumstances which tend to increase our knowledge of the properties of the latter, I shall here describe this experiment.

Experiment on Camphor with sulphuric Acid.

The effects produced on camphor by sulphuric acid have been but very superficially examined; for all that has hitherto been stated amounts to this, that camphor is dissolved by sulphuric acid, that a brown or reddish-brown solution is formed, and that the camphor is precipitated unchanged from this solution by water. These facts, however, only relate to a certain period of the operation, for if this be long continued, other effects are produced, which I shall now describe.

A. To one hundred grains of pure camphor put into a small glass alembic, one ounce of concentrated sulphuric acid was added. The camphor immediately became yellow, and gradually dissolved, during which, the acid progressively changed to brownish-red, and afterwards to brown. At this period, scarcely any sulphureous acid was evolved, but in about one hour the liquid became blackish-brown; much sulphureous acid gas was then produced, and continued to increase during four hours, when the whole appeared like a thick black liquid, at which period not any odour or appearance of camphor could be perceived, but only that of the sulphureous acid. After two days, during which time the alembic had not been heated, there did not appear any alteration, unless that the production of sulphureous gas was much diminished. The alembic was then placed in a sand-bath moderately warm, by which, more of the sulphureous gas was obtained, but this also soon began to abate. After the lapse of two other days, I added gradually six ounces of cold water, by which the liquid was changed to reddish-brown, a considerable coagulum of the same colour subsided, the odour of sulphureous gas, which in some measure had still prevailed, was immediately annulled, and was succeeded by one which resembled a mixture of oils of lavender and peppermint.

The whole was then subjected to gradual distillation, during which, the water came over strongly impregnated with the odour abovementioned, accompanied by a yellowish oil which floated on the top of it, and which, as far as could be ascertained, amounted to about three grains.

B. When the whole of the water was come over, there was again a slight production of sulphureous gas. I then added two ounces of water, which I drew off by distillation, but did not obtain any of the vegetable essential oil which has been mentioned, nor did the odour of it return, I therefore continued the distillation until a dry blackish brown mass remained; this was well washed with warm distilled water, by which, however, nothing was extracted; but when two ounces of alcohol were digested on it during twenty-four hours, a very dark brown tincture was formed.

The residuum was digested with two other ounces of alcohol in like manner, and the process was repeated until the alcohol ceased to act.

The residuum had now the appearance of a compact sort of coal in small fragments, it was then well dried, and after exposure to a low red heat in a close vessel, weighed fifty-three grains.

C. The different portions of the solution formed by alcohol were added together, and being distilled by means of a water-bath, a blackish brown substance was obtained, which had the appearance of a resin or gum with a slight odour of caromel, and weighed 49 grains.

The products therefore which were thus obtained from 100 grains of camphor when treated with sulphuric acid, were,

	Grains.
A. An essential oil which had an odour somewhat resembling a mixture of lavender and peppermint, about -	3
B. A compact and very hard sort of coal in small fragments - - - - -	53
C. And a blackish brown substance of a resinous appearance - - - - -	49
	105

From this statement it appears, that there was an increase in the weight amounting to five grains, which I attribute partly to oxygen united to the carbon, and partly to a portion of water so intimately combined with the last product, that it could not be expelled from it by heat without subjecting it to decomposition. The properties of this substance were as follows:

1. It was extremely brittle, had somewhat of the odour of caromel, the flavour was astringent, and it speedily dissolved in cold water, and formed with it a permanent dark brown solution.

2. This solution yielded very dark brown precipitates by the addition of sulphate of iron, acetite of lead, muriate of tin, and nitrate of lime.

3. Gold was copiously precipitated by it from its solution in the metallic state: and

4. By solution of isinglass, the whole was completely precipitated, so that after three or four hours, a colourless water only remained.

The precipitate was nearly black, and was insoluble in boiling water; from which property, as well as from the effect produced upon prepared skin by the solution, it was evident, that the substance thus obtained from camphor, was a variety of the artificial tanning matter, much resembling that which may be obtained from resinous bodies by means of sulphuric acid. But it must be observed, that this sort of tanning substance seems to act less powerfully on skin, than that which is prepared from carbonaceous substances by nitric acid, and the precipitate which the former produces with solution of gelatine is more flocculent and less tenacious, than that which in like manner is formed by the latter.

It is however remarkable, that when a small quantity of nitric acid was added to the solution of the substance obtained from camphor, and when after evaporating it to dryness, the residuum was dissolved in water, a reddish brown liquid was formed, which acted in every respect similar to the tanning substance obtained from the varieties of coal by nitric acid.

§ VII.

From the experiments which have been related, it appears, that three varieties of the artificial tanning substance may be formed, viz.

1st. That which is produced by the action of nitric acid upon any carbonaceous substance, whether vegetable, animal, or mineral.

2dly. That, which is formed by distilling nitric acid from common resin, indigo, dragon's blood, and various other substances; and,

3dly. That which is yielded to alcohol by common resin, elemi, asa fœtida, camphor, &c. after these bodies have been for some time previously digested with sulphuric acid.

Upon these three products I shall now make a few remarks, which I have hitherto postponed, in order that the account of the experiments might not be interrupted.

The first variety is that which is the most easily formed; and from some experiments which were purposely made, I find that 100 grains of dry vegetable charcoal afford 120 of the tanning substance; but as it is extremely difficult completely to expel moisture, or even the whole of the nitric acid which has been employed,* an allowance of about three or four grains ought to be made, so that after this deduction we may conclude, that 100 grains of vegetable charcoal yield 116 or 117 of the dry tanning substance.

The proportions of the constituent parts of this substance I have not as yet ascertained; but from the manner by which it is produced, carbon is evidently the base of it, and is the predominating essential ingredient.

From § III. experiment F. it also appears, that the other component parts are oxygen, hydrogen, and nitrogen; for when the artificial tanning substance was distilled, ammonia and carbonic acid were obtained, exclusive of a very small portion of a yellow liquor, which stained the upper part of the retort, and which, from its tenacity and insolubility in water and alcohol, appeared to be of an oily nature.

As I had taken every precaution respecting the charcoal which had been employed, I was at first induced to consider the above facts as almost positively demonstrative of the presence of hydrogen in charcoal, but upon farther reflexion, and upon weighing some of the circumstances which attend the formation of the artificial tanning substance, I still feel on this point very considerable doubt; for I have constantly observed, that diluted nitric acid, acts upon charcoal more effectually, in the formation of the tanning substance, than when it is employed in a concentrated state; and it appears therefore very probable, that hydrogen may have been afforded by a portion of water decomposed during the process. For admitting that the new compound (formed by the action of nitric acid upon charcoal) may possess a certain degree of affinity for hydrogen, this being exerted simultaneously with the affinity for oxygen possessed by nitrous gas, may (especially when the last is in a nascent state) effect a decomposition of a portion of water, the hydrogen of which would therefore enter into the composition of the tanning substance, whilst the oxygen would supply the place of part of that which had been taken from the nitric acid.

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* The most effectual method of expelling the nitric acid, is to reduce the tanning substance to powder, and repeatedly evaporate different portions of distilled water from it in a glass or porcelain basin.

Many of the properties of the tanning substance prepared from coal by nitric acid are very remarkable, particularly those which have been noticed in § III. experiments F. and G.; for surely it is not a little singular, that this substance when burned should emit an odour so very similar to animal matter, notwithstanding that the tanning substance had been prepared from pure vegetable charcoal. And again in experiment G. the portion which had not been precipitated by solution of isinglass, was, when dried, found to possess a strong vegetable odour very analogous to oak bark, although charcoal is inodorous, and isinglass very nearly so.

But, after all, the most extraordinary properties of this substance are certainly those which so nearly approach it to the vegetable principle called *tannin*; for it perfectly resembles this principle by its solubility in water and in alcohol, by its action upon gelatine and upon skin, by the effects which it produces upon metallic solutions, upon those of the earths, and of the alkalis.

The sulphuric and muriatic acids also affect the solutions of it as they do those of *tannin*; and the only marked difference which as yet has been found in the characters of the artificial substance and of *tannin*, is, that the former is produced, whilst the varieties of the latter are more or less destroyed by nitric acid. This, for the present at least, must draw a line of separation between them, but we must not forget, that even the varieties of *tannin*[*] do not accord in the degree of destructibility.

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[*] I shall here venture to state some ideas which have occurred to me on the probable cause and mode of the formation of tannin.

Mr. Biggin has proved, that similar barks when taken from trees at different seasons, differ as to the quantities of tannin contained in them. (Phil. Trans. 1799. p. 259.)

Mr. Davy also observes, "that the proportions of the astringent principles in barks vary considerably according as their age and size are different."

"That in every astringent bark the interior white bark (which is the part next to the alburnum) contains the largest quantity of tannin. The proportion of extractive matter is generally greatest in the middle or coloured part; but the epidermis seldom furnishes either tannin or extractive matter."

Moreover Mr. Davy remarks "that the white cortical layers are comparatively most abundant in young trees, and hence their barks contain in the same weight a larger proportion of tannin than the barks of old trees." Phil. Trans. 1803, p. 264.

We find, therefore,

1st. That the proportion of tannin in the same trees is different at different seasons.

2dly. That tannin is principally contained in the white cortical layers, or interior white bark which is next to the alburnum or new wood: and

3dly. That these white cortical layers are comparatively most abundant in young trees, and that their barks consequently contain in the same weight more tannin than the barks of old trees.

I shall not make any remarks on the first of these facts, as it accords with other similar effects, which are the natural consequences of the processes and periods of vege-

The second variety of the tanning substance is obtained from a great number of vegetable bodies, such as indigo, dragon's blood, common resin, &c. &c. by digesting and distilling them with nitric acid. It is not, therefore, quite so readily prepared as that which was first described, and its relative quantity, when compared with that of the substance employed to produce it, is less.

As resin and some of the other bodies do not afford it until they have been repeatedly treated with nitric acid, and as during each operation nitrous gas is produced, whilst the strength of the acid which comes over is diminished, it seems almost

tation; but the second and third appear to be important; for they prove that tannin is principally formed, or at least deposited, in the interior white bark, which is next to the alburnum or new wood; so that in the very same part where the successive portions of new wood are to be elaborated and deposited, we find the principal portion of tannin.

It should seem, therefore, that there is an intimate connexion between the formation of new wood and the formation of tannin in such vegetables as afford the latter; and this idea is corroborated when the chemical nature of these substances is considered.

From experiments made on the ligneous substance of vegetables, or the woody fibre, it appears to be composed of carbon, oxygen, hydrogen, and nitrogen, but of these its principal and essential ingredient is carbon.

In like manner carbon is unquestionably the basis and principal ingredient of tannin. Considering, therefore, that both of these substances consist principally of carbon, that tannin is secreted in that part of barks where the formation and deposition of new wood take place, and that the quantity of tannin is the most considerable in young trees, and seems therefore to keep pace with their more vigorous growth and consequent rapid formation of wood, it appears very probable that those vegetables which contain tannin, have the faculty of absorbing more carbon and of the other principles than are immediately required in the formation of the different proximate vegetable substances, especially the woody fibre: that this excess, by chemical combination, becomes tannin, which is secreted in the white interior bark: that in this state it is a principle peculiarly fitted to concur by assimilation to form new wood: that it is therefore subsequently decomposed at the proper period, and is employed in the evident, that this tanning substance is formed in consequence of part of the oxygen of the nitric acid becoming combined with the hydrogen of the original body, so as to form water; and the carbon being thus in some measure denuded, is rendered capable of being gradually acted upon by the nitric acid in a manner nearly similar to that, which takes place when it has been previously converted into coal.

The colour of the precipitates which this tanning substance yields with gelatine is constantly pale or deep yellow, whilst that of the precipitates formed by the first variety is always brown; I am therefore induced to believe, that the different colours of the precipitates produced by the varieties of tannin depend on the state of their carbon.

When resin and the other bodies were treated as above described with nitric acid, the quantity obtained of the tanning substance was much less than when an equal quantity of coal was employed, or even when these bodies had been previously converted into coal in the humid way by sulphuric acid.

The cause of this seems to be, that a number of other products are simultaneously formed, all of which require more or less of carbon as a constituent ingredient, so that, in consequence of the affinities which prevail under the existing circumstances,

formation of the new wood: that there is not a continual accumulation of tannin in the vegetables which afford it, as it is successively formed in and with the white cortical layers, and is successively decomposed by concurring to form new wood: and, lastly, that as the vegetable approaches more nearly to the full maturity of its growth, when wood is less rapidly and less plentifully formed, so in like manner less tannin is secreted, for the fabric being nearly completed, fewer materials are required.

Such I am inclined to suspect, from the facts which have been adduced, to be the cause and mode by which tannin is formed in oaks and other vegetables, but I make this statement only as a probable conjecture, which may be refuted or confirmed by future observations.

some bodies by treatment with nitric acid afford but little, and others none of the tanning substance.

The greatest proportion of this substance was yielded by indigo, common resin, and stick lac.

The quantity obtained from asa fœtida and gum ammoniac was less.

Benzoin, balsam of Tolu, balsam of Peru, and dragon's blood, were inferior to the former in this respect, so that the developement or rather production of benzoic acid[*] appeared partly to counteract the formation of the tanning substance.

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[*] The expression "*production of benzoic acid*" may appear objectionable, and I shall therefore take this opportunity to observe, that I much suspect the present established opinion respecting the balsams and benzoic acid to be erroneous. For the balsams are defined as bodies composed of resin and benzoic acid; consequently the latter, when obtained in a separate state, is considered as an original ingredient or educt.

I am however inclined to a contrary opinion, for I consider the balsams as peculiar substances, which, although nearly approaching to the nature of resins, are nevertheless different in respect to the original combination of their elementary principles, which combination however is with much facility modified by various causes, and especially by a certain increase of temperature, so that a new arrangement of the elementary principles takes place, part being formed into resin, and part into benzoic acid.

Many facts appear more or less to support this opinion; for whether benzoic acid is obtained by simple sublimation, or by merely digesting benzoin in boiling water, according to GEOFFROY's method, or by the addition of lime, as recommended by SCHEELE, or by employing alkalis in a similar manner, nothing positive can be inferred from any of these operations to prove that benzoic acid is obtained as an educt, but rather the contrary, when we reflect on the affinities which are most likely to prevail under the circumstances of the different processes, and on the variable proportions of the benzoic acid; and although benzoic acid has been discovered in the urine of infants, in that of many adults, and constantly in that of graminivorous quadrupeds, such as the camel, the horse, and the cow, (*Système des Connoissances Chimiques*, par FOURCROY, 4to edit. Tome IV. p. 158;) yet all this certainly appears to be in favour of its being a chemical product.

I have observed, when benzoin, balsam of Tolu, and balsam of Peru, were dissolved but oxalic acid when formed in any considerable quantity, seemed *absolutely to prevent* the formation of this substance; for whilst abundance of the former was obtained from gum arabic, tragacanth, manna, and guaiacum, not any of the latter could be produced.

Common liquorice appears at first to be an exception, but from the smallness of the quantity and the colour of the precipitate which it produced with solution of isinglass, I am almost convinced that the tanning substance was formed by the action of the nitric acid on a portion of uncombined carbon,

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in sulphuric acid, that a great quantity of beautifully crystallized white benzoic acid was sublimed during digestion; and as it is produced in so very pure a state by this single and simple operation, I would recommend a trial of the process to those who prepare benzoic acid for commerce; but I am not certain whether this mode may prove more economical than those which at present are employed.

When dragon's blood, however, was treated in the same manner with sulphuric acid, I could not obtain a particle of benzoic acid; nor did I succeed much better when I had recourse to lime, according to SCHEELE's process; for although a considerable quantity of the substance was thus rendered soluble in water, yet by the addition of muriatic acid I obtained only a slight appearance of benzoic acid accompanied by a copious precipitate of red resin, notwithstanding that the solution had acquired a powerful and peculiar balsamic odour.

But in a former part of this Paper I have stated, that when dragon's blood was dissolved in nitric acid, and afterwards evaporated to dryness, it yielded about 6 *per cent.* of benzoic acid. Now if this had been originally present in dragon's blood in the state of benzoic acid, some stronger evidence of it might reasonably have been expected in each process, but this not being the case, I am inclined to consider it as produced, and not educed, by the action of the nitric acid on the original principles of the dragon's blood; and I am also persuaded that similar but more general effects take place when benzoin or any of the balsams are subjected to the different processes by which benzoic acid is obtained; so that to me, this last seems to be as much a chemical product, as the oxalic, the acetous, and other of the vegetable acids.

The succinic acid also appears to be a product and not an original ingredient of amber.

which being in a state approaching to coal, is probably the cause of the blackness of the common liquorice.

As the formation of the tanning substance has been my principal object, I have not thought it necessary to enter at present into too minute a detail of other particulars, and have therefore only thus cursorily noticed some of the principal effects produced by nitric acid on the resins, balsams, &c. Those however who are conversant with chemistry, will undoubtedly perceive that these effects deserve to be accurately investigated, and that the resins, balsams, gum resins, and gums, should be regularly examined by every possible method, not merely on account of the individual substances which may become the subjects of experiment, but because there is reason to expect that from such an investigation, medicine, with the arts, and manufactures, may derive many advantages, whilst the mysterious processes and effects of vegetation may very probably receive considerable elucidation.

Concerning the third variety of the tanning substance, which is produced by the action of sulphuric acid on the resins, gum resins, &c. I shall here add but little to that which I have already stated in the latter part of the second section of my first paper, and in the account which I have lately given of an experiment on camphor.

This variety appears to be uniformly produced during a certain period of the process, but by a long continuance of the digestion, I have reason to believe that it is destroyed.

Substances, such as the gums, which afford much oxalic acid by treatment with other acids, do not apparently yield any of this tanning substance.

The energy of its action on gelatine and skin is certainly inferior to that of the first variety, into which however (as we have seen) it may easily be converted by nitric acid.

From the mode of its formation, there does not appear to be any positive evidence that it contains nitrogen like the first and second varieties, and perhaps the absence of nitrogen may be the cause of its less powerful action; this I have not as yet ascertained, but it is my intention more particularly to notice in a future Paper the general properties of this substance.

XXII. *On the Discovery of Palladium; with Observations on other Substances found with Platina. By* William Hyde Wollaston, *M. D. Sec. R. S.*

Read July 4, 1805.

Having some time since purified a large quantity of platina by precipitation, I have had an opportunity of observing various circumstances in the solution of this singular mineral, that have not been noticed by others, and which, I think, cannot fail to be interesting to this Society.

As I have already given an account of one product obtained from that ore, which I considered as a new metallic substance, and denominated Rhodium, I shall on the present occasion confine myself principally to those processes by which I originally detected, and subsequently obtained another metal, to which I gave the name of *Palladium*, from the planet that had been discovered nearly at the same time by Dr. Olbers.

In the course of my inquiries I have also examined the many impurities that are usually mixed with the grains of platina, but I shall not think it necessary to describe minutely substances which have already been fully examined by others.

§ I. *Ore of Iridium.*

I must however notice one ore, that I find accompanies the ore of platina, but has passed unobserved from its great resemblance to the grains of platina, and on that account is scarcely to be distinguished or separated from them, excepting by solution of the platina; for the grains of which I speak are wholly insoluble in nitro-muriatic acid. When tried by the file, they are harder than the grains of platina; under the hammer they are not in the least degree malleable; and in the fracture they appear to consist of laminæ possessing a peculiar lustre; so that although the greater number of them cannot, as I have before observed, be distinguished from the grains of platina, the laminated structure sometimes occasions an external form by which they may be detected. With a view to be absolutely certain that there exist grains in a natural state, which have not been detached by solution from the substance of the grains of platina, I have separated from the mixed ore as many as enabled me to ascertain their general composition.

Their most remarkable quality is their great specific gravity, which I have found to be as much as 19 5, while that of the crude grains of platina has not, in any experiment that I have made, exceeded 17,7. From this circumstance it might naturally be conjectured that they contain a greater quantity of platina than the grains in general; by analysis, however, they do not appear to me to contain the smallest quantity of that metal, but to be an ore consisting entirely of the metals that were found by Mr. TENNANT in the black powder which is extricated by solution from the grains of platina, and which he has called Iridium and Osmium. But, since the specific gravity of these grains so much exceeds that of the powder, which by my experiments has appeared to be, at the utmost, 14,2, I have thought it might deserve inquiry whether their chemical composition is in any respect different. For this purpose I have selected a portion of them, and have requested Mr. TENNANT to undertake a comparative examination, from whose well known skill in chemical inquiries, as well as peculiar knowledge of the subject, we have every reason to expect a complete analysis of this ore.

§ II. *Hyacinths.*

Among those bodies which may be separated from the ore of platina, in consequence of their less specific gravity, by a current of water or of air, there may be discerned a small proportion of red crystals so minute, that 100 of the largest I could collect weighed scarcely $\frac{8}{10}$ of a grain. The quantity which I possess is consequently too small for chemical analysis; but their physical properties are such as correspond in every respect with those of the hyacinth. I was first led to compare them with that stone by their specific gravity, which I conjectured to be considerable from their accompanying other substances, that appear to have been collected together solely by reason of their superior weight.

Like the hyacinth, these crystals lose their colour immediately and entirely when heated;[*] they also agree with it in their hardness, which is barely sufficient to scratch quartz, but is decidedly inferior to that of the topaz.

The principal varieties of their form may be very well understood by description.

1st. In its most simple state the crystal may be considered as a rectangular prism terminated by a quadrilateral obtuse pyramid, the sides of which sometimes arise direct from the sides of the prism; but,

2dly. The position of the pyramid is generally such that its sides arise from the angles of the prism. In this case the sides of the prism are hexagons.

3dly. It is more usual for the prism to have eight sides by truncation of each of its angles, and at each extremity eight additional surfaces occupying the place of the eight linear angles between the prism and terminating pyramid of the 2d variety. The complete crystal has then thirty-two sides.

4thly. The eight surfaces last mentioned, as interposed between the prism and pyramid, are sometimes elongated into a complete acute pyramid having eight sides arising from the angles of an octahedral prism.

The 3d form above described, corresponds so entirely with that given by the Abbé HAÜY * as one of the forms of the hyacinth or jargon, that I have little reason to regret my inability to obtain chemical evidence of the composition of these crystals.

Those, and other impurities, I usually separated, as far as was practicable, by mechanical means, previously to forming the solution of platina, which has been the principal object of my attention.

§ III. *Precipitation of Platina.*

When a considerable quantity of the ore had been dissolved, and I had obtained, in the form of a yellow triple salt, as much of the platina as could be precipitated by sal ammoniac, clean bars of iron were next immersed in the solution for the purpose of precipitating the remainder of the platina.

For distinction it will be convenient to call this, which in fact consists of various metals, the first metallic precipitate.

The treatment of this precipitate differed in no respect from that of the original ore. It was dissolved as before, and a portion of platina precipitated by sal ammoniac; but it was observable that the precipitate now obtained was not of so pale a yellow as the preceding. Nevertheless the impurity was in so small quantity, that the platina reduced from it by heat did not differ discernibly from that obtained from the purest yellow precipitate.

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* *Traité de Mineralogie*, Pl. XLI. fig. 17.—*Journ. des Mines*, No. 26, fig. 9.

At this time I found it advantageous to neutralize the solution with soda, and to employ a solution of green sulphate of iron for the precipitation of the gold, of which, I believe, a portion may always be obtained from the mixed ore; but I have observed in experiments upon any quantities of mere grains of crude platina carefully selected, that the smallest portion of gold could not be detected as a constituent part of the ore itself.

Bars of iron were subsequently employed as before for recovering the platina that remained dissolved, together with those substances which I have since found to accompany it.

The precipitate thus obtained, which I distinguish by the name of the second metallic precipitate, was to appearance of a blacker colour than the former, and was a finer powder.

As I was not at first prepared to expect any new bodies, I proceeded to treat the second precipitate, as the former, by solution and precipitation. But I soon observed appearances which I could not explain by supposition of the presence of any known bodies, and was led to form conjectures of future discoveries, which subsequent inquiry has fully confirmed.

When I attempted to dissolve this second metallic precipitate in nitro-muriatic acid, I was surprised to find that a part of it resisted the action of that solvent, notwithstanding any variations in the relative proportions or strength of the acids employed to form the compound, and although the whole of this powder had certainly been twice completely dissolved.

The solution formed in this case was of a peculiarly dark colour, and when I endeavoured to precipitate the platina from it by sal ammoniac, the precipitate obtained was small in quantity, and, instead of being yellow, was of a deep red colour, arising from an impurity which I did not at that time understand, but which we since know, from the experiments of Mr. DESCOTILS, is occasioned by the metal now called iridium.

The solution, instead of being rendered pale by the precipitation of the platina, retained its dark colour in consequence of the other metals that remained in solution; but, as I had not then learned the means of separating them from each other, and as the quantity of fluid which accumulated occasioned me some inconvenience, I decomposed it by iron, as in the former instances, and formed a third metallic precipitate, which could more commodiously be reserved for subsequent examination.

In this last step I committed an error which afterwards occasioned me considerable difficulty, for I found that a great part of this precipitate consisting of rhodium was unexpectedly rendered insoluble by this treatment, and resembled the residuum of the second metallic precipitate abovementioned.

As I have already communicated to this society, in my Paper upon rhodium, the process by which I subsequently avoided this difficulty, I shall at present return to a previous stage of my progress, and relate the means by which I first obtained palladium in my attempts to analyze the second metallic precipitate.

§ IV. *Separation of Palladium.*

There was no difficulty in ascertaining the presence of lead as one of the ingredients of this precipitate, by means of muriatic acid, which dissolved lead and iron and a small quantity of copper. It was equally easy to obtain a larger portion of copper by dilute nitrous acid, with which it formed as usual a blue solution. But when I endeavoured to extract the whole of the copper by a stronger acid, it was evident, from the dark brown colour of the solution, that some other metallic ingredient had also been dissolved. I at first ascribed this colour to iron; but, when I considered that this substance had been more slowly acted upon than copper, I relinquished that hypothesis, and endeavouring to precipitate a portion of it by a clean plate of copper, I obtained a black powder adhering to a surface of platina on which I had placed the solution. As this precipitate was soluble in nitric acid, it evidently consisted neither of gold nor platina; as the solution in that acid was of a red colour, the metal could not be either silver or mercury; and as the precipitation of it by copper excluded the supposition of all other known metals, I had reason to suspect the presence of some new body, but was not fully satisfied of its existence until I attempted the precipitation of it by mercury.

For this purpose I agitated a small quantity of mercury in the nitrous solution previously warmed, and observed the mercury to acquire the consistence of an amalgam. After this amalgam had been exposed to a red heat, there remained a white metal, which could not be fused before the blow pipe. It gave a red solution as before in nitrous acid; it was not precipitated by sal ammoniac, or by nitre; but by prussiate of potash it gave a yellow or orange precipitate; and in the order of its affinities it was precipitated by mercury but not by silver.

These are the properties by which I originally distinguished palladium; and by the assistance of these properties I obtained a sufficient quantity for investigating its nature more fully.

There were, however, various reasons which induced me to relinquish the original process of solution in nitrous acid and precipitation by mercury; for although I found the metal thus obtained to be nearly pure, the necessity of agitating the solution with the mercury was very tedious, and the waste was also considerable; for in the first place it seemed that nitrous acid would not extract all the palladium from any quantity of the second metallic precipitate, neither would mercury reduce the whole of what was so dissolved. I therefore substituted a process dependent on another of its properties. I had observed that this metal differed from platina in not being precipitated from nitro-muriatic acid by nitre or by other salts containing potash; for although a triple salt is thus formed, this salt is extremely soluble, while that of platina on the contrary requires a large quantity of water for its solution. On that account a compound *menstruum* consisting of nitrate of potash dissolved in muriatic acid is unfit for the solution of platina, but dissolves palladium nearly as well as common nitro-muriatic acid in which there is no potash present.*

In five ounces of muriatic acid diluted with an equal quantity of water, I dissolved one ounce of nitre, and formed a solvent for palladium that possesses little power of acting on platina, so that by digesting any quantity of the second metallic precipitate till there appeared to be no farther action, I procured a solution from which by due evaporation were formed crystals of a triple salt, consisting of palladium combined with muriatic acid and potash. These are the crystals which I have on a former occasion * mentioned as exhibiting a very singular contrast of colours, being bright green when seen transversely, but red in the direction of their axis; the general aspect, however, of large crystals is dark brown.

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* I have found that gold may also be dissolved with equal facility by the same solvent, and nearly in the same proportion. Ten grains of nitre added to a proper quantity of muriatic acid are sufficient for sixteen grains of either gold or palladium.

From the salt thus formed and purified by a second crystallization, the metal may be precipitated nearly pure by iron or by zinc, or it may be rendered so by subsequent digestion in muriatic acid.

§ V. *Reasons for thinking Palladium a simple Metal.*

From the consideration of this salt alone I thought it highly probable that the substance combined in it with muriate of potash was a simple metal, for I know of *no instance in chemistry of a distinctly crystallized salt containing more than two bases combined with one acid.* I nevertheless endeavoured by a suitable course of experiments to obviate all probable objections. After examining by what acids it might be dissolved and by what reagents it might be precipitated, I combined it with various metals, with platina, with gold, with silver, with copper, and with lead; and when I had recovered it from its alloys so formed, I ascertained that, after every mode of trial it still retained its characteristic properties, being soluble in nitrous acid, and precipitable from thence by mercury, by green sulphate of iron, by muriate of tin, by prussiate of potash, by each of the pure alkalis, and by hydrosulphurets.

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* Phil, Trans. 1804, p. 428.

The precipitate obtained in each case was also found to be reducible by mere heat to a white metal, that, except in very small quantities, could not be fused alone by the blowpipe, but could very readily be fused with sulphur, with arsenic, or with phosphorus, and in all other respects resembled the original metal.

The only hypothesis, on which I thought it possible that I could be deceived, arose from the recollection of the error, which subsisted for a few years, respecting the compound formerly called siderite. It was possible that some metallic or other fixed acid might unite too intimately with either a known or an unknown metal to be separated by the more common simple affinities. I consequently made such attempts as appeared best calculated to disunite a compound so constituted.

Having boiled the oxide with pure alkalis, and found it to be unaltered, I thought the affinities of lime or lead might be more likely to detect the presence of the phosphoric or of any known metallic acid; and accordingly I made various attempts by muriate and nitrate of lime, as well as by nitrate of lead, to effect a decomposition of the supposed compound. In the experiment on which I placed the greatest reliance, I poured liquid muriate of lime into a solution of palladium in nitro-muriatic acid, and evaporated the mixture to dryness, intending thereby to expel any excess of acid that might have been left in the solution, and to render either phosphate of lime, or any compound of lime with a metallic acid, insoluble in water. The residuum however was very readily dissolved by water, and consisted merely of muriate of lime and muriate of palladium, without any appearance of decomposition.

When I found all my endeavours directed to that end wholly unsuccessful, I no longer entertained any doubt of this substance being a new simple metal, and accordingly published a concise delineation of its character; but by not directing the attention of chemists to the substance from which it had been extracted, I reserved to myself an opportunity of examining more at leisure many anomalous phenomena, that had occurred to me in the analysis of platina, which I was at a loss to explain, until I had learned to distinguish those peculiarities, that I afterwards found to arise from the presence of rhodium.

§ VI. *Additional Properties of Palladium.*

In my former Paper on that subject I also added some observations upon the properties and origin of palladium, describing only such a mode of obtaining it from platina as should avoid the introduction of any unnecessary ingredient which might possibly be misinterpreted, and omitted one of the most distinguishing properties of palladium, by means of which it may be obtained with the utmost facility by any one who possesses a sufficient quantity of the ore of platina.

To a solution of crude platina, whether rendered neutral by evaporation of redundant acid, or saturated by addition of potash, of soda, or ammonia, by lime or magnesia, by mercury, by copper, or by iron, and also whether the platina has or has not been precipitated from the solution by sal ammoniac, it is merely necessary to add a solution of prussiate of mercury, for the precipitation of the palladium. Generally for a few seconds, and sometimes for a few minutes, there will be no appearance of any precipitate; but in a short time the whole solution becomes slightly turbid, and a flocculent precipitate is gradually formed, of a pale yellowish-white colour. This precipitate consists wholly of prussiate of palladium, and when heated will be found to yield that metal in a pure state, amounting to about 4 or 5 tenths *per cent.* upon the quantity of ore dissolved.

The prussiate of mercury is peculiarly adapted to the precipitation of palladium, exclusive of all other metals, on account of the great affinity of mercury for the prussic acid, which in this case prevents the precipitation of iron or copper; but the proportion of mercury does not by any means influence the quantity of palladium, for I have in vain endeavoured, in the above experiment on crude platina, to obtain a larger quantity of palladium than I have stated by using more of the prussiate of mercury, or to procure any precipitate by the same means from a solution of pure platina.

The decomposition of muriate of palladium by prussiate of mercury is not effected solely by the superior affinity of mercury for the muriatic acid, but is assisted also by the greater affinity of prussic acid for palladium; for I have found that prussiate of palladium may be formed by boiling a precipitated oxide of palladium in a solution of prussiate of mercury.

The prussiate of mercury is consequently a test by which the presence of palladium may be detected in any of its solutions; but it may be worth observing, that the precipitate obtained has not in all cases the same properties. In general, this compound is affected by heat similarly to other prussiates, but when the palladium has been dissolved in nitrous acid and precipitated from a neutral solution by prussiate of mercury, the precipitate thus formed has the property of detonating when heated. The noise is similar to that occasioned by firing an equal quantity of gunpowder, and accordingly the explosion is attended with no marks of violence unless occasioned by close confinement. The heat requisite for this purpose is barely sufficient to melt bismuth, consequently is about 500° of Fahrenheit. The light produced is proportionally feeble, and can only be seen in the absence of all other light.

In endeavouring to dissolve a piece of palladium in strong colourless nitric acid for the purpose of forming the detonating prussiate, I found that, although the acid shortly acquired a red colour surrounding the metal, the action of the acid was extremely slow, and I was surprised to observe a fact that appears to me wholly singular: the metal was taken up without any extrication of nitrous gas; and this seemed to be the cause of the slow solution of this metal, as there was not that circulation of the fluid, which takes place in the solution of other metals until the acid is nearly saturated.

As the want of production of gas appeared to retard the solution of palladium, I tried the effect of impregnating a quantity of the same acid previously with nitrous gas, and observed its action to be very considerably augmented, although the experiment was necessarily tried in the cold, because the gas would have been expelled by the application of heat.

Beside those properties which are peculiar to palladium there are others, not less remarkable, which it possesses in common with platina. I have on a former occasion mentioned that these metals resemble each other in destroying the colour of a large quantity of gold. Their resemblance, however, in other properties is not less remarkable, more especially in the little power they possess of conducting heat, and in the small degree of expansion to which they are liable when heated.

For the purpose of making a comparison of the conducting powers of different metals, I endeavoured to employ them in such a manner, that the same weight of each metal might expose the same extent of surface. With that view I selected pieces of silver, of copper, of palladium, and platina, which had been laminated so thin as to weigh each 10 grains to the square inch. Of these I cut slips $\frac{4}{10}$ of an inch in breadth, and four inches long; and having covered their surfaces with wax, I heated one extremity so as to be visibly red, and, observing the distance to which the wax was melted, I found that upon the silver it had melted as far as $3\frac{1}{4}$ inches: upon the copper $2\frac{1}{2}$ inches: but upon the palladium and upon the platina only 1 inch each: a difference sufficient to establish the peculiarity of these metals, although the conducting power cannot be said to be simply in proportion to those distances.

In order to form some estimate of the comparative rate of expansion of these metals, I rivetted together two thin plates of platina and of palladium; and observing that the compound plate, when heated, became concave on the side of the platina, I ascertained that the expansion of palladium is in some degree the greater of the two. By a similar mode of comparison I found that palladium expands considerably less than steel by heat; so that if the expansion of platina between the temperatures of freezing and boiling water be estimated at 9 parts in 10,000, while that of steel is known to be about 12, the expansion of palladium will probably not be much more or less than 10, or one part in 1000 by the same difference of temperature.

It must, however, be acknowledged that the method I have pursued is by no means sufficient for determining the precise quantity of expansion of any substance; but I have not been induced to bestow much time on such an inquiry, since the extreme scarcity of palladium precludes all chance of any practical utility to be derived from a more accurate investigation.

XXIII. *Experiments on a Mineral Substance formerly supposed to be Zeolite; with some Remarks on two Species of Uran-glimmer. By the Rev.* William Gregor. *Communicated by* Charles Hatchett, *Esq. F. R. S.*

Read July 4, 1805.

THIS mineral is raised in a mine called Stenna Gwyn, in the parish of St. Stephen's, in Branwell, in the county of Cornwall; the principal production of which is the compound sulphuret of tin, copper, and iron.

Description.

Two species of this mineral are found, assuming a marked difference in external character.

The first and most common one consists of an assemblage of minute crystals, which are attached to quartz crystals, in tufts, which diverge from the point of adherence, as from a centre. These tufts vary, as to the number of crystals, of which they are composed, and are light and delicate in the forms which they assume, or they are grouped together according to a variety of degrees of proximity and compactness. Sometimes they fill the whole cavity of a stone, with little or no interruption; in other specimens they are seen partially spreading over the sides and pointed pyramids of quartz crystals.

In some cases these grouped tufts adhere very pertinaciously to the stone which bears them; in others, they are easily separable, in comparatively large pieces, from the quartz, he impressed form of which the pieces thus separated, retain. The surface of these, which was in immediate contact with the quartz, exhibits the several minute crystals of which the mass consists, matted together in various directions.

These crystalline assemblages are, in general, white; a nearer inspection of the individual crystals proves that they are transparent. Sometimes they are stained of a yellowish hue by ochry water.

The size of these crystals varies considerably in different specimens. Sometimes they assume the appearance of a white powder raised up in small heaps, upon the surface of the stone, to which they adhere. In other specimens they resemble a tender down. And the larger sort varies, in relative size, in the proportion, perhaps, in which a human hair, horse-hair, and a hog's bristle, severally differ from each other in magnitude. They seldom exceed a quarter of an inch in length. The figure of these crystals is not easily ascertainable, on account of their minuteness. By the help of a very powerful microscope, they appear to consist of four-sided prisms; where these are broken off, the section exhibits a rhomboidal, approaching indeed to an elliptical figure, from the circumstance of the angles of the prism being worn away; but that the prism itself is rhomboidal, cannot be inferred from hence, unless we could be certified, that the section were at right angles with the axis of it.

Imbedded amongst these crystals two species of crystalline laminæ are frequently discoverable: the one consisting of parallelopipedon plates with truncated angles, applied to each other, of a green colour of various tints, from the emerald to the apple-green: the other species, consisting of an assemblage of square plates, which vary in thickness. The angles of the several square laminæ, which are applied to each other are not always coincident. They are of a bright wax yellow. The sides of the largest of these square laminæ is about a quarter of an inch. This last species is frequently found adhering to the sides of quartz crystals, in the cavities of granite.

The other species of this mineral consists of an assemblage of crystals closely compacted together in the form of mammillary protuberances, in general, of the size of small peas, intimately connected with each other. A stratum of these about $\frac{1}{8}$ of an inch thick is spread upon a layer of quartz, in the cavities or fissures of a species of compact granite. The striæ of which these mamillæ consist diverge from a centre, like zeolite. Some of the individual striæ, in some cases, overtop their fellows, in these globular assemblages, and evidently assume, on their projecting points, a crystallized form.

A.

(1.) The detached crystals of the former species are easily reduced to powder, of a brilliant whiteness. At the temperature 56° of FAHRENHEIT, its specific gravity was found to be 2,22.

(2.) The hardness of the more compact species is sufficient to scratch calcareous spar. At the temperature 55°, its specific gravity was 2,253. It does not imbibe water.

(3.) Some of the crystals exposed, on charcoal, to the flame of the blowpipe suddenly and strongly driven upon them, decrepitate: if they are gradually exposed to the flame they grow opaque, and become more light and tender: but they show no signs of fusion under the strongest heat.

(4.) The phosphate of soda and ammonia takes up a piece of this mineral without effervescence, but it swims about the fused globule, unaltered. Borax dissolves a fragment of a crystal, and the globule remains transparent.

(5.) Some of this mineral, reduced to a fine powder, was mixed with about half its weight of pounded quartz, and kneaded with water into a ball: but as soon as the mass became dry, all cohesion was destroyed, and it fell into powder.

(6.) Sulphuric acid, poured upon some of it, caused no effervescence, nor was there any perceptible vapour extricated.

(7.) Some of the pulverized crystals were put into a crucible of platina, and sulphuric acid was poured upon them. The crucible was covered with a piece of glass, and placed in warm sand. On examination of the crucible and its contents, after some time, it appeared that the greater part of the mineral had been dissolved, but the surface of the glass cover was not in the least affected.

(8.) Some of the crystals were introduced into a small glass retort, to which a receiver was adapted. The retort was exposed to the heat of a charcoal fire. A fluid distilled over into the receiver, which had a peculiar empyreumatic smell. It changed litmus-paper to a faint red. It produced no change in a solution of nitrate of silver; but it caused a white precipitate in a solution of nitrate of mercury. I attributed these phænomena, at the time, to a small bit of the feather with which I had swept the powder into the retort, and which, I thought, had fallen into it. A slight whitish crust was also produced in the neck of the retort, but the smallness of the quantity did not admit of examination.

(9.) Some of this mineral exposed to a red heat, for about ten minutes, lost in weight at the rate of $25\frac{5}{6}$ *per cent.* Another portion, exposed to a stronger heat for more than an hour, lost $30\frac{3}{4}$ *per cent.* This operation was performed in a crucible of platina; the cover of which gave some indications as if a slight portion of the finer parts had been volatilized.

Some of the compact species, after exposure to a red heat for one hour, experienced a diminution in weight of 30 *per cent.*

(10.) The sulphuric, muriatic and nitric acids, aided by a long digesting heat, effect nearly a complete solution of this substance. The quantity of the undissolved residuum is diminished in proportion to the purity of the mineral employed.

(11.) The nitrate of silver, as well as the muriate of barytes, produce no change in the solution of this substance in nitric acid.

(12.) The solutions of this substance in muriatic and nitric acids, cannot be brought to crystallize.

B.

(1.) I selected some of the crystals of this substance, as free as it was possible from extraneous matter. 50 grains grossly pounded were exposed, in a platina crucible, to a red heat for one hour. They weighed, *whilst still warm*, $35\frac{7}{8}$ grains, which is a loss of $28\frac{1}{4}$ *per cent.* 25 grains of the same parcel, from which I had taken the former, exposed to a heat of longer continuance and greater intensity, were diminished in weight, at the rate of $30\frac{3}{4}$ *per cent.*

(2.) The powder still preserved its pure whiteness. It was transferred into a matrass, and nitric acid poured upon it, which soon began to act upon it. The matrass was placed, for many hours, in a digesting heat. A solution of the whole of the substance, except a small portion, was effected. I added a few drops of muriatic acid, and continued the digestion.

(3.) The acid was now diluted with distilled water, and poured off from the residuum, which consisted partly of a fine spongy earth, and partly of fragments of quartz. It was caught on a filter and sufficiently edulcorated. The last portion of edulcorating water dropped through the filter of an opalish hue.

The residuum, dried and exposed to a red heat, for ten minutes, $= \frac{3}{16}$ of a grain, $\frac{1}{16}$ of which consisted of fragments of quartz, $\frac{1}{32}$ was found to be silica, and $\frac{3}{32}$ alumina.

C.

(1.) The clear solution and the edulcorating water were poured into a large matrass and boiled, and whilst boiling, the contents were precipitated, in white flakes, by ammonia.

(2.) When the ammonia had ceased to produce any further precipitate, the clear fluid was decanted, and assayed with carbonate of ammonia. But its transparency was not in the least disturbed.

(3.) This clear fluid, together with the edulcorating water, with which the subsided precipitate had been washed, was gradually evaporated. When its volume was considerably diminished, a separation of a spongy earth took place, more copiously than I had reason to expect, and the quantity of it was still further increased by a few drops of ammonia. This earth, thus separated, was sufficiently edulcorated, and added to the former precipitate.

(4.) The fluid was again evaporated, and at last transferred to a crucible of platina, and the salt reduced to a dry state: on redissolving this salt in distilled water a minute portion of earthy matter was separated, which, after edulcoration, was added to the rest. The fluid from which it had been separated, and the edulcorating water, were again evaporated to dryness, and the ammoniacal salt expelled by heat, in a platina crucible.

(5.) After the crucible had been made red hot, it was examined. I discovered on the bottom of it, some traces of earthy matter, and some spots, which had a glassy appearance. Water boiled upon it, dissolved nothing; from which circumstance, the absence of both of the fixed alkaline salts may be inferred. Neither did nitric acid produce any alteration. A few drops of sulphuric acid effected a solution of the substance, which adhered to the bottom of the crucible. Ammonia precipitated from it a small quantity of earth, which was transferred to the rest, and the sulphate of ammonia and edulcorating water were again evaporated and expelled by heat. A few spots of the same glazing still appeared. I had observed the same phænomenon in a former experiment: but in that, as well as in the present instance, the substance was in too small a quantity to become the subject of experiment.

D.

(1.) Upon the precipitate (C 1), and the earths collected at different times, whilst they were in a moist state, I poured a solution of potash in alcohol mixed with distilled water; in a short time, the greater part of it was dissolved.

The clear solution was decanted, and the undissolved sediment was transferred to a bason of pure silver, and boiled with a solution of potash.

(2.) When the potash ceased to act upon it, it was diluted with distilled water and decanted from a brown powder, which had subsided. This powder edulcorated, dried, and ignited weighed $\frac{7}{16}$ of a grain; $\frac{1}{4}$ of a grain was alumina, $\frac{3}{32}$ silica, and $\frac{3}{32}$ oxide of iron.

E.

(1.) The solution effected by potash was decomposed and redissolved by muriatic acid, and the contents of the solution were precipitated by ammonia. The subsided precipitate was edulcorated.

(2.) The fluid and the edulcorating water were evaporated to dryness, and redissolved in distilled water. Here again, to my surprise, a separation took place of a white earth, more abundant than is usual in cases where ammonia is employed as a precipitant.

(3.) This earth and the precipitate were edulcorated with distilled water, until it ceased to affect a solution of nitrate of mercury. Collected, dried, and ignited, for one hour it weighed *whilst still warm* 32 $\frac{1}{16}$.

F.

(1.) This earth was placed in a crucible of platina, and repeatedly moistened with sulphuric acid, which was abstracted from it in the sand bath; distilled water effected the solution of the whole, except a white powder which weighed, after ignition, 2$\frac{7}{32}$ grains. It was proved to be silica.

(2.) This solution was now mixed with some acetat of potash and gradually evaporated; large and regular crystals of alum were from time to time formed. A small portion of silica which weighed after ignition $\frac{1}{32}$ of grain was deposited:

some sulphat of lime also made its appearance, which washed with diluted alcohol and dried in a low heat $=\frac{1}{16}$ of a grain.

(3.) A portion of the fluid remained which neither the addition of potash nor the lapse of many weeks could induce to crystallize. Suspecting that it might contain glucine, I precipitated the contents by carbonat of ammonia, added to excess, and shook the mixture repeatedly and strongly. The precipitated earth was collected and the fluid boiled, but it was found to contain nothing but a minute portion of alumina.

(4.) The edulcorated earth was redissolved in sulphuric acid, except $\frac{5}{8}$ of a grain of ignited silica.

The solution was mixed with a little potash, and gradually evaporated. Sulphat of of lime was separated at several times and after long intervals, which sufficiently washed and dried in a low heat $=\frac{9}{32}$. Some silica also separated, but too minute in quantity to be ascertained by weight. The remaining fluid at length crystallized into regularly formed alum.

(5.) The whole, therefore, of the $32\frac{1}{16}$ (E. 3.) consisted of alumina except $2\frac{7}{8}$ of silica, and the lime contained in $\frac{23}{32}$ of sulphat of lime, which may be estimated about $\frac{3}{16}$ of a grain; the alumina, therefore, $= 29$; the alumina in B. and D. $=\frac{11}{32}$; the silica in B, D, and F, $=3\frac{1}{16}$; the oxide of iron (D.) $=\frac{3}{32}$, and lime F, $\frac{3}{16}$; the volatile parts of this substance $= 15\frac{3}{8}$ in the 50 grains employed.

The sum total of these is - - - -	$47\frac{1}{16}$
Loss - - - - - -	$2\frac{15}{16}$
	50

I have subjected these crystals, as well as the harder species of this mineral, to analysis by means of direct solution in sulphuric acid, and have found in each case the same fixed ingredients, viz. alumina, a small portion of silica, and a very minute quantity of lime. Both these latter ingredients are, I think, essential to the composition of this fossil, as I have always discovered them in the purest specimens. In this mode of analysis I experienced the same difficulty and tediousness of delay in bringing the last portions of the solution to crystallize into alum. This anomalous circumstance I have reason to attribute to a particular combination, which takes place between the sulphat of alumina and lime, silica, and potash. In my examination of the compact species there was no appearance of the sulphat of lime until the last; and in every experiment, previously to the fresh appearance of crystals of alum, that had been long delayed, silica and sulphat of lime were deposited.

I forbear entering into any further details concerning my former experiments on this curious fossil, as I have reason to think that it will still require a more particular and minute examination, on account of another ingredient which eluded my notice, and which may possibly impart to it its peculiar character. The scarcity of it has been hitherto a great bar to my experiments; I shall record, however, a few facts which I have lately observed, in the hope that at a future time I may be able to resume my examination of it.

I was induced to pay more attention to the volatile ingredients of this substance.* With this view, I introduced some of the crystals into a small retort, adapted a receiver unto it, and exposed the retort to a charcoal fire. The neck of the retort was soon covered with moisture, which passed into the receiver; and I observed a white crust gradually forming in the arch and neck of the retort.

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* Mr. HUMPHRY DAVY, whose well known skill and sagacity have probably rendered the researches of another person superfluous, had, I found, been engaged in the analysis of a mineral which is thought to be identical with the subject of these observations. He informed me that he had observed a peculiar smell, and acid properties in the water distilled from the substance which he examined.

On examination of the fluid in the receiver, it was found to have the same empyreumatic smell that I had observed before. It resembles very much the smell which that fluid is found to have which is distilled from the white crust that surrounds flint as a nucleus.

It changed litmus paper to a faint reddish hue. It produced no change on a solution of nitrat of silver, and scarcely a perceptible one, on that of nitrat of mercury.

The crust formed in the neck of the retort consisted of thin scales, which after the vessel had been dried, were disposed to separate from the glass in some places, but in others they firmly adhered unto it. They were opaque, like white enamel, and reflected the colours of the rainbow. A portion of this subtance exposed to the flame of the blow-pipe upon charcoal turned at first black, and then melted into a globule, that exhibited somewhat of a metallic splendor which soon grew dull. This substance is soluble in water; on evaporation of it, it assumes, at the edges of the fluid, a saline appearance, which, as the moisture evaporates, becomes earthy, opake, and white. Some of the solution changed litmus paper to a faint red. Lime and strontian waters produce in it white clouds, which a drop of nitric acid removes. Muriats of lime and barytes produce no change in it. Nitrat and acetat of barytes disturb its transparency, the effect produced by the latter is more evident. Nitrat of silver produces no effect, but nitrats of mercury and lead cause copious precipitates, which are white, and soluble in nitric acid. Phosphat of ammonia and soda produced a white precipitate. Oxalat, tartrite, and prussiat of potash did not affect it, nor did sulphat of soda. Ammonia was dropped into it, but the fluid preserved its transparency. But carbonat of ammonia instantly caused a white precipitate, which was not redissolved by an excess of the precipitant; upon some of this subsided precipitate a concentrated solution of potash was poured and shaken with it, but it was not sensibly diminished. But if after edulcoration it be dissolved in nitric acid, and potash be added, no precipitate is produced.

Carbonat of potash causes a white precipitate when dropped into the aqueous solution of the scaly sublimate.

The supernatant fluid was poured off and gradually evaporated, but it became repeatedly turbid, nor could I by means either of the filter or alcohol prevent a recurrence of the same effect. Nearly the same result takes place when carbonat of ammonia is used as the precipitant.

Some of the white scales were moistened with sulphuric acid. No vapour arose.

Some of the precipitate obtained by means of carbonat of potash from the watery solution of this substance, was, after sufficient edulcoration, dissolved in sulphuric acid; the solution, on due evaporation, produced permanent crystals, some of which resembled alum, but others seemed to differ from it in external character. Ammonia decomposed the solution of them in water, and a few drops of liquid potash dissolved the precipitated earth. The quantity was too small for further experiment.

If distilled water be poured into the retort and boiled in it, so as to dissolve what adheres to the neck and cavity of it, a further solution is effected, but differing in some measure from the solution of the sublimate collected from the neck of the vessel. This latter solution is found to contain lead. If nitric or muriatic acid be poured into the retort, so as to dissolve what *still* remains adhering to it, the presence of lead becomes more evident. Whence does this metal arise? I have reason to believe that it arises from the glass retort, which is corroded by the acid of the fossil extricated by heat. But what acid is it? It does not seem to be either the phosphoric or fluoric acids, the latter of which became the first object of my suspicion.

The opinion which Mr. DAVY suggested to me seems more probable, that it is of vegetable origin. Oxalic acid, on the authority of BERGMAN, may be volatilized; yet some of its properties are very extraordinary and do not accord with this idea.

I decomposed the watery solution of the scales by nitrat of lead, and after a sufficient edulcoration of the subsided precipitate, I dropped upon it some sulphuric acid. No fumes were perceptible. The sulphat of lead was separated by the filter, and the clear fluid, which passed through it, was gradually evaporated; small crystallizations were formed, the figure of which I could not ascertain; some of them were exposed to the flame of the blowpipe in a gold spoon; they did not burn to coal, nor give out any empyreumatic smell nor fuse, but they assumed an earthy appearance.*

---

* I subjected some of the Barnstaple mineral, with which Mr. RASHLEIGH kindly furnished me out of his cabinet, to experiment, with a view of ascertaining whether it would produce the same volatilized saline crust, as the Stenna Gwyn fossil, and I found that it did.

Uran-glimmer.

I shall add a few desultory remarks upon the yellow and green crystals, which frequently accompany this fossil.

I considered them to be the two species of Uran-glimmer, which had been examined by the celebrated KLAPROTH.

The yellow cubic crystals are light. Their specific gravity, taken at temperature 45° FAHRENHEIT, was 2,19.

Exposed to the flame of the blowpipe on charcoal, they decrepitate violently. A piece of this substance is taken up by phosphate of ammonia and soda, without effervescence, and communicates a light emerald-green colour to the fused globule.

By exposure to a red heat, this substance loses nearly a third part of its weight. It then becomes of a brassy colour

It is soluble in the nitric and muriatic acids: but I could procure no crystallized salt from the solution of either of them.

By evaporation to dryness, and redissolving the mass, some silica is separated.

A.

(1.) A certain quantity of the yellow crystals were dissolved in nitric acid. Muriatic and sulphuric acids successively dropped into the solution produced no sensible change. The contents of the solution were precipitated by ammonia, in white clots, mixed with some of a yellowish hue. Ammonia, added in excess, betrayed no sign of the presence of copper.

(2.) The ammonia, on evaporation, was found to have held a portion of the mineral in solution. A fresh portion of ammonia dissolved more, but in a less quantity, at each succeeding affusion of it.

(3.) The precipitate, which had resisted the ammonia, was boiled in a silver crucible, with a solution of potash in alcohol, diluted with distilled water, and a considerable portion of the substance was dissolved by it: the potash and the ammonia had dissolved rather more than half of the fixed ingredients of it.

(4.) The edulcorated residuum, which was of a dirty yellow colour, was transferred to a crucible of platina, and moistened with sulphuric acid, which was abstracted from it, in the sand-bath. The brownish-gray mass was elixated with distilled water, which dissolved nearly the whole of it. The residuum consisted of a white heavy powder, which, tried in different ways, was found to be sulphate of lead.

(5.) The solution effected by sulphuric acid was greenish. On evaporation, a salt was produced, of uncommon brilliancy, resembling scales of mica, or silver leaf. These diminished in quantity at every fresh solution and evaporation, and at last they could not be reproduced; but a confused crystallized mass remained. How far the platina crucible may have contributed to this phænomenon I cannot ascertain.

(6.) The solution of the saline mass was precipitated by potash, of a dark brown colour. The potash held nothing in solution. I redissolved the precipitate in nitric acid, and precipitated the solution by ammonia, of a bright yellow colour, peculiar to the oxide of uranium, with which it agreed in other properties.

(7.) What was dissolved by ammonia (2.) amounted to nearly $\frac{1}{6}$ part of the fixed ingredients. It was white, inclining to ash-colour. It tinged phosphate of soda and ammonia of a light green. It was soluble in sulphuric acid, except a few gelatinous flakes. The solution was greenish; gradually evaporated, it shot into a number of minute stellated crystallizations, which were circular, and consisted of rays diverging from a centre. They were, in general, colourless: a few of them were tinged of a smoke-colour. They soon became deliquescent. Upon evaporation, the same crystallizations were produced. After a time, some detached, regular, and permanent crystals were formed, which were colourless. Their figure I could not accurately ascertain. They were exposed to a red heat in a platina crucible. No ammoniacal vapour was perceptible. The crystals melted into opaque globules: some of these were transferred to a small glass, and distilled water was poured upon them. No solution took place, apparently: on shaking the glass, the globules fell to pieces into gelatinous flakes, which were white. Some of the supernatant fluid was tried with muriate of barytes, which produced a cloud. But neither ammonia nor prussiate of potash caused any change in it. It is soluble also in nitric acid: the solution formed a confused crystallized mass, which soon became deliquescent. Zinc, immersed in it, caused the separation of white gelatinous flakes. Iron caused no change. Ammonia and potash threw down white precipitates, a portion of which were redissolved. The carbonates of soda, potash, and ammonia produced white precipitates. Prussiate of potash threw down the contents of the solution in distinct flakes, of the colour of mahogany; and the solution of galls in alcohol caused a light yellow powder to subside. It is soluble also in muriatic acid; the solution is a very dilute green. It requires an excess of acid to hold the substance in solution; which, after a time, deposits crystalline grains of a yellowish colour, which require a large quantity of water to dissolve them.

Acetic acid does not dissolve this powder.

(8.) What was dissolved by potash (3.) was of an ISABELLA colour: it was tried with nitric, muriatic, and sulphuric acids, neither of which could dissolve the whole of it. What resisted the two former acids was found to be silica. That which remained undissolved by the latter, was silica and sulphate of lead. Evaporation of the latter solution, betrayed also the presence of lime, in the state of sulphate. The nitric and muriatic solutions, on evaporation, deposited nitrate and muriate of lead; and sulphuric acid dropped into them produced a small quantity of sulphate of lime.

The nitrate and muriate of lead were decomposed by sulphuric acid, and the lead reduced on charcoal.

Ammonia precipitated what remained in these solutions, and redissolved a part of the precipitates, which agreed in properties with that substance before mentioned (2.); the remainder was of a brighter yellow. But I could not bring the solution of it in nitric acid to crystallize.

B.

(1.) Some of the yellow crystals, which had not the slightest appearance of being contaminated with extraneous matter, were dissolved in sulphuric acid. Silica was separated; and the presence of lime and lead proved by the appearance of their respective sulphates.

(2.) If sulphate of ammonia is dropped into a solution of this mineral in nitric or muriatic acids, no change takes place, *immediately*. But on evaporation, a yellowish crust is deposited, which is insoluble in water. A solution of carbonate of soda in water, boiled on it, becomes yellowish-brown, and the greater part of it is dissolved. The residuum, which is white, is reduced on charcoal to a globule of lead. What the carbonate of soda had dissolved was found to be oxide of uranium. Sulphuric acid *alone*, does not produce this deposited crust.

(3.) Some perfectly pure crystals were dissolved in muriatic acid. Some silica was separated. A few drops of sulphuric acid were dropped into the solution, which produced no immediate change: on evaporation a white powder separated, which consisted in part of sulphate of lime. The remainder, exposed to the flame of the blowpipe, was reduced to globules of lead.

The solution was decomposed by ammonia, which redissolved a part of the precipitate; and, after edulcoration, the precipitate was dissolved by nitric acid, and precipitated again by ammonia, which held a less quantity in solution. The edulcorated precipitate was now boiled with a solution of carbonate of soda, which dissolved a large portion of it. The solution was yellowish-brown, and contained oxide of uranium. What was undissolved by the carbonate of soda was dissolved in sulphuric acid, and seemed to be the same substance as that which the ammonia held in solution. A. (2.)

The scarcity of this beautiful mineral has precluded me from operating on such a sufficient quantity, as a regular and rigid analysis required.

The substance, which is held in solution by ammonia, has some peculiar properties that seem to distinguish it from uranium. And if this mineral be the Uran-glimmer, I have certainly detected the oxide of lead, lime, and silica in it, which have not hitherto been considered as ingredients of that fossil. The green crystals differ in no respect from the yellow, except in containing a little of the oxide of copper.

Creed,
June 14th, 1805.

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